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NATURAL SCIENCE:

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NATURAL SCIENCE:

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NOTES AND COMMENTS.

Scientific Dinners.

NOW that the festive season is upon us, a few remarks on Scientific Dinners may not be out of place.

Most of the learned Societies meet once a year to celebrate their "anniversary," in a more or less convivial fashion. This they do, not in the style of the "good old days" of the "three-bottle" men—the process is in a measure reversed. Instead of a plain dinner and a large amount of wine, the custom is to have a very elaborate dinner and a moderate amount of wine. The cost may be much about the same.

In the meanwhile the composition of the societies has undergone considerable alteration. In old days, Science was the pursuit, or the pastime, for the most part of those well-to-do—of leading professional men, of clergy, and men of independent means. Nowadays Science owes its progress as much, perhaps more, to poor men than to the comparatively rich. Yet the annual dinners are practically restricted to the latter, on account of the guinea (or more) that is charged. This is not as it should be. To the younger workers, as a rule, the cost is prohibitory. These social gatherings should be representative: whereas, under present circumstances, it seems as if we had a Christmas dinner, and denied a place to the children and to our poor but hard-working relations. A change is needed, so that rich and poor, old and young, may meet, and equally find a welcome.

We read in the *Life of Edward Forbes* that during the meeting of the British Association held in Birmingham in 1839, "He and other young naturalists, disliking the irksomeness and expense of the ordinary, adjourned to a small tavern, adorned with the sign of the Red Lion. There they dined daily at small expense, on beef cooked

in various fashions, moistened with sundry potations of beer, and enlivened by joke and song—in contradistinction to the endless dishes and wines and formality of the 'big wigs.'"

"And in after years, when he had arrived at the zenith of his reputation, and British geology had conferred upon him the highest honours it had to bestow, his antipathy to men of buckram remained as strong as ever. He could see no reason why a President of the Geological Society should cease to be a 'Red Lion,' and so he chanted his songs and cracked his jokes as merrily as he had done in the little Birmingham tavern when he was only beginning to be known."

The Red Lion dinner, alas! is not what it used to be—with the formalities and expenses that seem to attend the introduction of "war-paint."

In making these remarks we have no desire to advocate a return to the customs of the past, nor to impair in any degree the true dignity of Science; but we fully believe that if the festive gatherings of our learned Societies were arranged on more economical lines, they would much more adequately fulfil the purpose for which they are instituted. In reference to the customs of the past, it will be of interest to append the following account of a Royal Society Club Dinner a century ago, taken from a translation of Faujas St. Fond's *Travels in England, Scotland, and the Hebrides*, 1799 (vol. i., pp. 48–51):—

"About forty members of the Royal Society have been, for more than twenty-five years, in the habit of dining annually in one of the taverns of London. Each member has the right of bringing to this club two visitors, whom he chooses, among foreigners or the friends of the Royal Society of his own acquaintance. The president may bring a greater number, and can select whoever he pleases for guests.

"We sat down to table about five o'clock. Sir Joseph Banks presided, and filled the place of honour. No napkins were laid before us; indeed there were none used; the dinner was quite in the English style.

"A member of the club, who is a clergyman (I believe it was the astronomer Maskelyne), made a short prayer, and blessed the company and the food. The dishes were of the solid kind, such as roast beef, boiled beef and mutton prepared in various manners, with abundance of potatoes and other vegetables, which each person seasoned as he pleased with the different sauces which were placed on the table.

"The beef-steaks and the roast beef were at first sufficiently drenched by large quantities of strong beer, called porter; it was drank out of cylindrical pewter pots, which are, by some, thought preferable to glasses, perhaps because they enable one to swallow a whole pint at a draught.

"This prelude being finished, the cloth was removed, and a handsome and well-polished table was covered, as if it were by magic, with a number of fine crystal decanters, filled with the best port, madeira and claret; this last is the wine of Bordeaux. Several glasses were distributed to each person, and the libations commenced on a grand scale, in the midst of different kinds of cheeses, which,

rolling in mahogany cases from one end of the table to the other, provoked the thirst of the drinkers.

"To give more liveliness to the scene, the president announced the health of the prince of Wales; this was his birth-day. We then drank to the elector palatine, who was that day to be admitted a member of the Royal Society. The same compliment was next paid to us foreigners, of whom there were five present.

"The members of the club afterwards saluted each other, one by one, with a glass of wine. According to this custom, one must drink as many times as there are guests, for it would be thought a want of politeness in England to drink to the health of more persons than one at a time.

"A few bottles of champaign soon put all the company in good humour. The tea came next, with butter, marmalade, and all its usual accompaniments; coffee followed, humbly yielding precedence to the tea, though it be the better of the two. In France, we commonly drink only one cup of good coffee after dinner; in England they drink five or six times that quantity of the most detestable kind.

"Brandy, rum, and some other strong liquors closed this philosophic banquet, which terminated at half-past seven, as there was to be a meeting of the Royal Society at eight o'clock. Before we left the club-room, the names of all the guests were written on a large sheet of paper, and each of us paid seven livres four sols French money: this was not dear.

"I repaired to the Society along with Sir Joseph Banks,—Cavendish, Dr. Maskelyne,—Aubert, and Sir [Henry] Englefield; we were all pretty much enlivened, but our gaiety was decorous.

"Doubtless, I should not wish to partake of similar dinners it they were to be followed by settling the interests of a great nation, or discussing the best form of government; such a conduct would neither be wise nor prudent; but to meet, to celebrate the admission of an elector palatine, who has, besides, much merit, to a learned Society, is not a circumstance from which any inconvenience can result."

Circumstances, as we have said, have changed much since the time of St. Fond's travels, and we can only hope that the conservatism of the older learned Societies will ere long be compelled to yield to the spirit of the age.

National Museums.

An article of some importance "On our National Art Museums and Galleries" is contributed by Sir Charles Robinson to the *Nineteenth Century* for December, 1892 (p. 1025), and many of his remarks will apply equally well to the National Scientific Collections. While fully appreciating the zeal with which the officers in charge of the various museums perform their duties, and while duly acknowledging the gratifying progress that is being made, Sir Charles laments the want of a central controlling organisation to direct the whole and prevent unnecessary duplication of specimens:—"It is little to say that in our museum system everything is in a chaotic state, everything drifts fortuitously; there is no central overruling and directive power, no bond of union, and scarcely any intercommunion between one

establishment and another—briefly, no definitely established system for the general governance of these institutions ; hence tacit rivalries, which sometimes develop into flagrant antagonisms. The bounds of jurisdiction or the several provinces of these institutions overlap in all directions, and their respective interests clash."

Everyone acquainted with the Natural History Departments of the British Museum and the allied public institutions of more recent foundation, will recognise how appropriately these remarks apply to them. The originally economic aims of Kew Gardens, for example, have expanded to such an extent that the Herbarium is in constant rivalry with that of the British Museum, separating collections which ought to be united, and duplicating collections in other instances. The Museum of Practical Geology, also, continually acquires fossils which are of little or no value for stratigraphical or economic purposes, and which, from their unique interest in an anatomical sense, ought to be placed with the purely scientific collections in Cromwell Road. When the Art Museums are re-organised, by all means let the Science Museums be similarly controlled, and the reformation will be not merely economical, but conducive to efficiency.

The Botanist in Winter.

Though so many of our native plants lie dormant in the winter, the time is not lost even for them. We perhaps go into the lanes or fields and see only withered stalks, or ripe fruit waiting to be picked or shaken off. We say to ourselves that the year is finished, and nothing more is to be done till life revives with the first warm days of spring. This idea, however, is quite mistaken, for the botanist who has the good fortune to be in the field in winter sees things that are quite unknown to the summer rambler. He soon learns that though life may be dormant, or even extinct, in the dead stalks, yet they are not therefore merely cumbering the ground and waiting for decay. In many of our native plants the adaptation of the means to the end does not cease with life, and the dying or dead stem is often modified to help to protect or disperse the seeds.

Winter botany has been little studied, but there is much to be learnt. For instance, the dead, unsightly umbellifers, so common in every hedge, are still playing their part. Instead of the green, flexible stem found in the summer, we see a stiff, elastic stalk, with thin whip-like tips, from which the fruit, separating from the axis, dangle by one corner. Anyone who has seen boys throw pellets of clay by means of a switch, and has afterwards forced his way through dead umbelliferous stems, like those of the Wild Chervil, will at once realise the great use of the elastic stalk, the whip-like tip, and the divided carpophore with its dangling fruit. Each passing animal and every breeze bends the stalk, which, springing back, tends to fling the fruit well beyond the limit of the ground exhausted by the

parent plant. Thus the elasticity of the dead stem may be as important to the species as any character in the living plant.

Many annual plants, however, have burrs, and in these species the dead stem is more flexible and tougher, so that it bends and rubs the fruit against the fur of an animal, or against our clothing. The different character of the dead stem will at once be recognised in the Wild Carrot, the ripe burr-like fruit of which curl inward, so that they cannot be shaken off, but come away a few at a time when the plant is stroked by anything rough.

This is merely one example of what can be observed in late autumn and winter; but anyone who has visited a beech or oak wood during a gale in the fruiting season, will understand why such fruit grow on the tips of long flexible branches, instead of on thick stems. The stinging blow that can be given by an acorn under these circumstances will soon convince the naturalist of the important part played by the flexible branch. Even the gale that tears off large arms may be of great use to the species, though ruinous to the individual tree.

A Naturalist in the West Indies.

Mr. W. R. Elliott, who has been collecting Cellular Cryptogams during the last year in St. Vincent, Anguilla, and Dominica for the West India (Natural History) Exploration Committee, has met with considerable success. In his last expedition, he has made a prolonged examination of the highest peaks of Dominica, the most densely wooded and primitive of all the Antilles, except, perhaps, Hayti, "the black republic," of which very little is known. He has obtained a very large series of Hepaticæ which Mr. Spruce has undertaken to work out, while he has added to the large number of Fungi already collected and partly described by Mr. George Massee in the *Journal of Botany*.

Among other things of which Mr. Elliott has been in quest is the petrel, the Diablotin, supposed to occur only in Trinidad. He has found the holes frequented by the birds, and at the time of his last letter was awaiting the possible re-appearance of a stray specimen or two, since the season (November) had arrived at which this might be expected. He visited the Carib reservation of Salybia on the windward coast of the island, but his account adds little of note to that given of the expiring remnant of this people by Mr. Ober in his interesting *Camps in the Caribbees*, except that the race of true Caribs is now in much the same case as the Diablotin. It would be of interest, and even a negative result would be of some value, if a local naturalist of St. Vincent or Grenada were to examine, at the proper season, some of the higher, less accessible, and seldom visited island peaks among the Grenadines whence reports have come at different times of the appearance of a bird resembling the Diablotin.

The Square-Mouthed Rhinoceros.

It has been considered that the square-mouthed, or so-called White Rhinoceros (*R. simus*) is already extinct. Recently, however, Mr. F. C. Selous has written to our contemporary, the *Field*, to say that a few of these magnificent animals still survive in a remote corner of Mashonaland. One of these last survivors has already been killed, with the view that its remains should find their way to the British Museum. We trust, however, that the authorities in Mashonaland will take care that the others are not molested, and may be allowed a chance of propagating their species.

We may take this opportunity of mentioning that although most zoologists have recognised the extreme specialisation of the molar teeth of this species, as exemplified by their very tall crowns, and the flat plane of wear of their grinding-surfaces, yet it does not appear to have been noticed that they differ from those of all other living Rhinoceroses by the presence of a thick investing coat of cement. Not only does this cover the outer surface of the tooth, but it likewise fills up both the main and the posterior valleys. Indeed, the molars of this species bear almost the same relationship to those of the Sumatran Rhinoceros as is presented by the molars of the Horse to those of the Anchithere.

In the extinct Woolly Rhinoceros (*R. antiquitatis*) the cement is present to a less degree in the molar teeth, which are of the same general structure; while in the allied *R. platyrhinus*, of the Siwalik Hills of India, there was, probably, also a certain amount of this constituent. In both these extinct species the teeth do not, however, appear to attain the extreme specialisation of the square-mouthed Rhinoceros, and it is accordingly difficult to regard the latter as representing a genus apart from the one including all the other existing species, which we should otherwise have been disposed to do.

The Science of Growth.

Readers of a recent article in Natural Science on the Anatomy and Development of the Brachiopoda, who were appalled by certain strange and fearful words, such as "nepionic," "ephebolic," and "nostologic," will be interested to learn that those terms have just been elucidated and improved by S. S. Buckman and F. A. Bather in a paper entitled "The Terms of Auxology" (*Zoologischer Anzeiger*, November 14 and 28, 1892, pp. 420 and 429). Growth and change, as the authors observe, do not stop in an animal when the embryonic stage has been passed; nor is the study of later stages of less importance than that of the earlier. Thus has arisen a new science, of which embryology is only a part; and, as was inevitable, definite names have been given to the various post-embryonic stages. Unfortunately, the names used were not only open to serious objection

on etymological grounds, but were used with too great laxity. As now revised, the technical terms are as follows :—*embryonic*, *brephic* (= infantine or larval), *neanic* (= adolescent), *ephebic* (= adult), and *gerontic* (= senile), the last being subdivided into *catabatic* (= declining) and *hypostrophic* (= atavic). These terms are restricted to stages in the growth of an individual. By prefixing the syllable *phyl-* they may be made to express stages in the history of a race; while the successive stages in the evolution of a character may be designated by the same terms with the addition of the prefix *morpho-*. Confusion of terms leads most surely to confusion of thought, and if the latter be in any way dispelled by the labours of Messrs. Buckman and Bather, we are bound to be grateful.

The Growth of Shells.

A most peculiar, and, as far as is at present known, unique mode of increase in a Gastropod shell, is described by Mr. B. B. Woodward in the last number of the *Proc. Zool. Soc. London* (1892, pp. 528—540, pls. 31, 32). Broadly speaking, the Gastropod shell may be looked upon as a more or less elongate conical tube, at the apex of which is the young shell, while increase takes place by the addition of fresh material at the other, open, extremity—the mouth. This tube is usually spirally coiled in the direction of a screw, the successive whorls touching one another. In those forms where the spire is low and the whorls are close together, the portion of the tube next the previously-formed whorl is sometimes largely, sometimes completely, dispensed with, and what was in the first instance the outermost wall of the tube becomes in the course of growth the dividing partition between the last whorl and its predecessor. When the shell is a very thick one, the presence of an equally stout internal partition (*paries*) between the whorls becomes unnecessary, and even inconvenient; hence it is very frequently to a great extent re-absorbed and reduced in thickness by the animal, as, for example, in *Conus*.

The Neritidæ advance a step further, and usually remove the greater part of the paries altogether, converting the shell into a single chamber, the only remnant of the dividing wall left being a portion near the mouth, to which one of the retractor muscles is attached. On the other hand, this remaining fragment of paries is usually strengthened, sometimes considerably, by an extra layer of shelly material. In extreme cases this deposit forms an independent projecting shelf (*septum*), standing out from the paries into the general cavity, between the former and the mouth of the shell. In these instances the septum forms the point of attachment for the retractor muscle, and the remnant of the paries is reduced to insignificant proportions or disappears entirely. The different species of *Neritina* and *Nerita* exhibit the various stages in the process.

Another feature of the Neritidæ is the presence of a thick shelly deposit (the *callus*) just without the mouth, on the surface of the last whorl. This deposit is thickest at the margin next the mouth, and generally at this point forms a ridge or even a thin shelf, or septum, which stretches partly across the mouth, reducing it in size. The full edge of this shelf is often serrate, sometimes coarsely so. As the animal grows and adds to its shell, still winding round in a spiral direction, these various deposits advance with it by the simple process of the addition of fresh material on their outer sides (using the word "outer" in its relation to the mouth and the direction of growth), and the removal of a corresponding amount on their inner sides.

Having prefaced so much, it is easy to understand the interest of what took place in the most eccentric Eocene member of the family, *Velates conoideus*, Lamk., as described by Mr. Woodward. In this species the growth of the young shell was perfectly normal, but when it had completed about $4\frac{1}{2}$ whorls a remarkable change ensued; it ceased to grow spirally, and increased by the addition of fresh layers of shell over the whole surface of the callus as well as round the margins of its mouth, and gained the additional internal space the animal required by the removal of a corresponding amount of shell on the inner side of the callus. It thus changed both its direction and axis of growth—and like the Irishman, it raised its roof by digging out the floor of its tenement. In a full-grown specimen, therefore, nearly half the shell and the internal septum were carved out of former callus, the layers of which can be seen in sections cutting across the walls and running round on the inner surface. A still more remarkable feature is presented in the disposition of the component plates which, in the callus, are so arranged as to provide, in anticipation, the strongest possible structure for the muscle-carrying septum, ultimately to be sculptured out from it. The presence of silica in the outer layer of the shell is also noteworthy, and it is interesting in this connection to remark that one of the earliest describers of this fossil gastropod, and the author after whom it is sometimes called—C. C. Schmidel—observed, in 1780, that this outer layer did not appear to consist entirely of lime.

The Classification of Bivalved Shells.

After the embryologist has made a certain advance in the study of development, and the comparative anatomist has progressed in the determination of homologous parts, the services of the systematic zoologist are required to express the results in orderly sequence. Classifications are thus merely temporary expedients—generalised retrospects, so to speak—and every advance demands some reconsideration. At the present time Professor Carl Grobben (of Vienna) is of opinion that some reform is necessary in the classification of the

bivalved mollusca; and he publishes the following scheme in the *Zoologischer Anzeiger* for October 24 (vol xv., pp. 373—375) as most in accordance with the existing state of knowledge:—

Class: Lamellibranchiata (Pelecypoda).

Sub-Class I.: Protobranchiata.—With double comb-shaped gills; hinge toothless or with interlocking denticulations of the hinge-border or taxodont. Families: Vlastidæ, Cardiolidæ, Antipleuridæ, Lunulicardiidæ, Præcardiidæ, Silurinidæ, Protomyidæ [*Solenomya*], Solenopsidæ, Grammysiidæ, Posidonomyidæ, Daonellidæ, Nuculidæ.

Sub-Class II.: Desmodonta.—With double lamellar gills; hinge-teeth wanting or irregular, arising in intimate connection with the ligament supports. Families: Pholadomyidæ, Myidæ, Anatinidæ, Panopæidæ, Septibranchia, Mactridæ, Pholadidæ, Gastrochænidæ.

Sub-Class III.—Ambonodonta. With double lamellar gills; hinge-teeth inclined backwards on the hinge-plate of the shell, variable in position, may be wanting.

Order 1. *Eutaxodonta*. With taxodont hinge. Family: Arcidæ.

Order 2. *Heterodonta*. With heterodont hinge. Families: Astartidæ, Crassatellidæ, Chamidæ, Lucinidæ, Cardiidæ, Tridacnidæ, Cyrenidæ, Cyprinidæ, Veneridæ, Solenidæ, Tellinidæ, Donacidæ.

Order 3. *Schizodonta*. With schizodont hinge. Families: Trigoniidæ, Najadæ.

Order 4. *Anisomyaria*. Hinge-teeth wanting, or if present, isodont or irregular; with two very dissimilar hinge-muscles or merely a single hinge-muscle. Families: Aviculidæ, Mytilidæ, Pinnidæ, Pectinidæ, Spondylidæ, Ostreidæ, Anomiidæ.

The Curvature of Plants.

Mr. Francis Darwin and Miss D. Pertz have lately been studying the artificial production of rhythm in plants, and the results appear in the last number of the *Annals of Botany*. The observations were made with the help of a new machine devised by the authors, which they call an intermittent klinostat. It differs from the ordinary klinostat in that movement is allowed only at regular intervals. The spindle is connected with a clock in such a way that a half rotation, *i.e.*, of 180 degrees, occurs at the end of every half-hour. Experiments were made with relation both to geotropic and heliotropic curvatures. In the former series, shoots of valerian and stalks of dandelion were used; these were fixed through a bored cork in test-tubes of water, which were attached horizontally to the spindle. The machine was placed opposite a window to cancel the effect of light in causing curvatures. When the clock was working, the influence of gravity on the growing shoot or stalk was, of course, reversed every half-hour, and it therefore received a stimulus of equal duration to curve towards opposite sides.

After the first few half-hourly intervals, the curvatures in either direction continued regularly for about thirty minutes. If the clock

were stopped, the rhythm was found to persist for a short time, the shoot or stalk actually curving in opposition to gravity for the half-hourly interval before finally obeying the impulse to grow downwards.

A similar effect was observed with heliotropic curvatures, for which seedlings of canary grass were used, the rotation being, of course, in a horizontal plane.

In one case, after the clock was stopped, the seedlings curved away from the light, for *two* half-hourly intervals, separated by one of curvature towards the light, so strongly were they imbued with the artificially-induced rhythm. The authors were evidently much struck with the result of their experiment; "to watch the movement of the free end of the shoot," they say, "and to see it reversed exactly at the expiration of half an hour is an experience so impressive as to compel belief. When a shoot is in a thoroughly rhythmic state, it is possible to prophesy to a minute at what time the reversal will take place." They compare the persistence of the rhythm after cessation of the stimulus to habit, like that acquired by a man who, after being regularly called for a time at 6 a.m. will awaken of his own accord at that hour. In the case, however, of the man, something in his surroundings, such as duration of light, may unconsciously affect him, but in the above experiments with plants, no such factor can have intervened, and the rhythm must be the result of an "internal chronometry," associated probably with the course of nutrition.

Recent Progress in the Study of Algæ.

The subject of the diseases of seaweeds is beginning to attract attention. So far, all that is known relates to the familiar *Vaucheria* galls and to the occurrence of Chytridieæ in *Sphacelaria*, *Cladostephus*, *Ceramium*, &c., as recorded by Magnus in the German North Sea Commission's Reports; while there are two papers by Miss Barton, one on galls in *Rhodymenia* caused by a copepod (*Journ. Botany*), and another on the malformations of *Ascophyllum* and *Desmarestia* (*Phycological Memoirs*). The thread-worm (*Tylenchus fucicola*), which causes the malformation of *Ascophyllum*, has been minutely described and beautifully figured by Dr. de Man in the recently issued *Festschrift* in honour of Professor Leuckart. Dr. Schmitz has described, at the British Association, Edinburgh, and in the *Botanische Zeitung*, certain tubercles of Florideæ, caused by the inevitable Bacteria; and a further contribution of great interest will shortly be made to the subject by Miss Frances Whitting, who is studying remarkable malformations of *Sarcophycus*, a Fucaceous Alga of the southern seas. This note of the literature of the subject may serve to call further attention to a very promising field of study.

A very admirable example of true methods in the study of Cryptogamic Botany is to be found in a memoir on the Algæ of P. K. A. Schousboe, contributed by M. Ed. Bornet to the *Société*

nationale des Sciences naturelles et mathématiques de Cherbourg (vol. xxviii., 1892). These Algæ were collected so long ago as 1815 29 by Peter Schousboe, Danish Consul at Morocco, and a part of them were bought by the King of Denmark, who placed them in the Botanic Garden of Copenhagen. The rest, together with a herbarium of Phanerogams, came into the hands of M. E. Cosson, and the late M. Thuret undertook and began the working out of the Algæ. He did not live to complete his labour, but sets were distributed by M. Kralik, under the name of *Algæ Schousbœanae*, and the British Museum possesses a very excellent one. M. Bornet has set to work on the complete series in Herb. Thuret, and the result is the admirable treatise just issued. The numerous phycologists who have found trouble in dealing with the *Algæ Schousbœanae* will be able to appreciate this authoritative publication, and especially the valuable critical notes on the species, as excellent and useful as any systematic work yet done by the great French botanist. A good deal of it will have particular interest to those who study seriously the distribution of our Channel Algæ.

Mr. James Ellis Humphrey, at present attached to the Agricultural Station at Amherst, Mass., is about to visit Jamaica for the purpose of working specially at the Algæ of that island. He intends to pay attention to the development of the multinucleate forms and in so rich a field is sure to meet with rewards.

Recent Progress in the Study of Fungi.

Among new mycological literature, the *Vegetable Wasps and Plant Worms*, by M. C. Cooke, furnishes an account of the Fungi that prey upon insects. His book is based on the privately-printed *Insect Bases of Fungoid Parasites*, by G. R. Gray (1858). Dr. Cooke brings it up to date in his own way, which is not remarkable for severe accuracy, but the result is a guide, at all events, to the literature of the subject.

Messrs. Ellis & Everhart's *North American Pyrenomycetes* has at last appeared and in a ponderous form. It is an excellent piece of work, and, so far as we have tested it, errs only in the references not being sufficiently exhaustive—especially the earlier historical references, if these may be so distinguished from the more modern.

Mr. Massee, whose activity in book production threatens Dr. Cooke's supremacy in this respect, has produced the first volume of his *British Fungus Flora* (Bell & Sons). Mixed with much that is both acutely and shrewdly critical, there is considerable credulity in the matter of taking references and statements for granted as correct. This kind of credulity may be beautiful as a matter of sentiment, and convenient in cases of haste, but it makes a vast difference to the value of a book.

An interesting and very comprehensive book on the truffle, by M. A. Chatin ("La Truffe," Baillière et Fils, Paris), has just been

published. It is a good example of the varying "form" displayed by a scientific author, his present effort being as successful as his *Anatomie Comparée* was the reverse.

IT is interesting to remark how the skeletons of animals that we have always hitherto regarded as typically Old World forms of life are still being discovered in the Tertiary formations of North America. The latest announcement is that of the discovery of a hyæna by Professor Cope in the Pliocene formation of Texas. It is an animal about as large as the common spotted hyæna, and is only known to differ from *Hyæna*-proper in the possession of a fourth premolar in the lower jaw. It is named *Borophagus diversidens* (*American Naturalist*, 1892, p. 1028). With the hyæna were also found two tortoises, one bird, one sloth, three mastodons, one peccary, three horses, one camel, a weasel, and a large cat. One of the horses—a true *Equus*—is remarkable for its small size, the teeth being no larger than those of a sheep.

SIR HERBERT MAXWELL and Mr. J. E. Harting, the Chairman and Secretary respectively of the Committee appointed by the Board of Agriculture to investigate the plague of field voles in Scotland, will shortly proceed to Thessaly to examine the results of Professor Loeffler's work in that region. As is well-known, the Professor has attempted to exterminate the Thessalian voles by placing in their food the germs of mouse typhus, and he claims to have been successful.

THE important monograph on the development of the macrurous Crustacea, by Professor W. K. Brooks and Mr. F. H. Herrick, to which we referred some time ago (vol. i., p. 243), has lately been issued in vol. v. of the *Memoirs of the National Academy of Sciences*. The other memoirs in the same volume relate to physical subjects.

WE have received from Mr. Hugh Fulton a photograph of the beautiful little land-shell, *Opisthostoma mirabile*, discovered a short time ago by Mr. A. H. Everett in North Borneo. In ornamentation this minute species rivals the *O. grandispinosa*, and several specimens have lately been obtained by Mr. Fulton.

IN the recently-issued number of the *Annals of Botany*, Dr. Schunck gives a short account of the work done on the chemistry of the green colouring matter of plants (chlorophyll) since the publication of his former paper on the subject in the volume for 1889.—In the same journal Mr. R. A. Rolfe describes a natural hybrid

between the frog orchis (*Habenaria viridis*) and the spotted orchis (*Orchis maculata*). It was found at Longwitton, in Northumberland, and shows, in a marked manner, a blending of characters derived from its two very distinct parents. The spur, for instance, is neither long nor short, but intermediate between the forms characterising the *Habenaria* and *Orchis* respectively. The hybrid receives the somewhat formidable name of *Habenari-orchis viridi-maculata*.

Attention has been directed to the subject of Lunar Volcanoes by Mr. J. B. Hannay (*Nature*, vol. xlvii., p. 7). He thinks that many of the craters look more like some disturbance in a semi-liquid surface than an accumulation of volcanic *débris;* and remarks that the rise and fall of a fused slag through holes in its solidifying crust, form features exactly like the craters in the moon. He suggests that, in the case of the moon, the rise and fall would be caused by the tidal motion of its still liquid interior. Mr. S. E. Peal also enquires (*Geological Magazine*, Nov., 1892, p. 501) whether the moon "could retain from the igneous-molten era, to its present *intensely* cold, airless and waterless condition its pristine surfacing, the very poles themselves being covered (it is urged) with large and small volcanoes, untouched by the hand of time." He has, however, written a book, which he speaks of as "my Theory of Lunar Surfacing by Glaciation."

The supply of natural gas and petroleum in Canada does not yet approach in importance that of the United States; we are glad to see, however, by the recently-issued First Report of the Bureau of Mines, 1891 [Ontario], that the industry is making steady progress. An Act has wisely been passed to prohibit the wanton waste of the by no means inexhaustible supply, and to provide for the compulsory plugging of unused bore-holes. The same report shows that nickel mining, of which so much was expected, progresses but slowly, the new alloys of nickel-steel not having yet come into use to any great extent. The ore is a mixture of copper-pyrites and nickeliferous pyrrhotite, usually associated with greenstone.

An interesting discovery of Lithographic Stone is recorded in the seventh volume of the *Mineral Resources of the United States*, lately issued by the U.S. Geological Survey. Material of excellent quality, comparing very favourably with Bavarian stone, has been found near Little Rock, Arkansas; and stone that has been tested and found serviceable, occurs at Fincastle, Virginia, and also in Blanco County, Texas. Preparations have been made for working the stone at the three localities. In England, trials for Lithographic

Stone have been made of slabs from the Lower Lias and White Lias (Rhætic) Beds, but not, so far as we know, with satisfactory results.

THE new part of the *Proceedings of the Geologists' Association* contains an interesting account, by Mr. W. J. L. Abbott, of the deposits and fossils found in excavations made for the foundation of the new Admiralty buildings at Whitehall. Full lists of the fossils are given, the most interesting finds being an undetermined species of water-tortoise (*Emys*), and leaves of the Arctic birch (*Betula nana*). This is the first time any Arctic plant has been found fossil in the Thames valley, and the water-tortoise in Britain was known only from Norfolk.

GEOLOGISTS will be glad to learn that a series of unpublished papers and notes by the late Professor Carvill Lewis, on the Glacial Geology of Great Britain and Ireland, will shortly be published as a small volume by Messrs. Longmans & Co. Dr. H. W. Crosskey is preparing an introduction.

A BIOGRAPHY of the late Sir Andrew Ramsay, who was for nine years Director of the Geological Survey of Great Britain, is being prepared by his successor, Sir Archibald Geikie.

A NEW monthly journal of Ornithology, the *Ornithologischen Monatsberichte*, is announced by Messrs. Friedländer & Son, of Berlin. The first number is to appear this month, and the annual subscription is 6 marks. The editor is Dr. Ant. Reichenow, and the main object of the journal appears to be the publication of brief preliminary notes on new researches, with a detailed index to current ornithological literature.

THE World's Fair at Chicago possesses an unusual interest from a Hygienic point of view. The probability is that cholera will break out again in Europe next spring. Whatever may be the vehicle of the cholera germs, it certainly tends to spread along the route where those who are affected have passed. It would certainly more or less affect the success of the Fair if intending visitors had any suspicion that dangers arising from this source were not entirely under control. There is also some reason to suppose that Chicago is well suited to become a sort of headquarter for this disease. In 1891 the typhoid fever death-rate of Chicago was twelve times as great as that of London. This state of the public health points to a water supply contaminated by germs, and it is natural to expect that, if the cholera germ by any means reaches Chicago, the water supply would be one of the most efficient means of spreading it.

One Jacob Horner has written a book called "Did a Hen or an Egg exist first? or My Talks with a Sceptic." One Mr. James Crompton has edited it, and the Religious Tract Society have published it in London in 1892. Mr. Jacob Horner says in the preface that he has found many of the artisans intelligent, that they are fond of an argument, that they want to understand the grounds of religion. "They like to get hold of a smart book with some dash and 'go' in it." Therefore has this book been written. It is a series of dialogues between an uncle and his nephew Tom Rod, "who has taken it into his head to be a Freethinker." In each round the stuffing is very agreeably knocked out of Tom till, at the end of the book, he writes a letter, the purport of which is that he can't come up to time again until he has been refilled. The stuffing is the very worst and oldest we have ever seen. It is, in fact, so bad, that it is hardly worth while pointing out that the nimble Uncle's blows are nearly all fouls. Take the first round as a sample. For brevity, we take the blows only and leave out the feints:—Uncle: Did a Hen or an Egg exist first? Tom objects to question. Uncle explains, and Tom admits that every egg comes from a hen, every hen comes from a chicken, and every chicken from an egg. Uncle puts in that Sir Charles Lyell says "You must explain the past by the present." Tom admits. Uncle: This must have held in the past. Tom admits. Therefore, first egg must have come from first hen, and so the first hen must have existed before the egg, and could not have come from an egg; and so Lyell's principle breaks down; you can't explain the past from the present. Stuffing badly out. There are 96 pages of this sort of thing. The last chapter is very appropriately entitled "Where are you going?"

Our first article this month relates to the life and works of Sir Richard Owen, who died on December 18. At the moment of going to press we regret to learn that the conspiracy of silence so long prevailing in certain quarters as to the great anatomist's writings, extended even to the arrangements for his burial. We have to record that a public funeral was not even offered, and Sir Richard Owen's remains were interred in Ham Churchyard in the presence of a small gathering of personal friends and official representatives of the scientific institutions of London.

I.

Owen.

I.—BIOGRAPHICAL NOTICE.

IT is with deep regret that we record this month the death of Sir Richard Owen. The ablest comparative anatomist of his time, Owen ranks with Darwin and Lyell as one of the greatest naturalists that England has yet produced.

Born at Lancaster eighty-eight years ago, Richard Owen received his early education at the local grammar school, and became, at the age of twenty, a student at Edinburgh University. His activity and talents enabled him to reap the fullest advantages offered by his teachers, among whom were Wm. Pulteney Alison, T. C. Hope, Robert Jameson, Alexander Monro (the third), and Alexander Barclay; and in 1825 he had already been elected President of the Hunterian Society, which he and others had formed a short time before. Leaving Edinburgh in 1826, Owen joined the medical school of St. Bartholomew's Hospital,where he came into contact with the celebrated John Abernethy. It was, it is interesting to note, to the Medical Society of this hospital that he presented his first published paper, "An Account of the Dissection of the parts concerned in the Aneurism, for the cure of which Dr. Stevens tied the Internal Iliac Artery," which appeared in the *Medico-Chirurgical Transactions* for 1830.

In the year 1826, he became a member of the Royal College of Surgeons, and contemplated entering the Naval Service. Fortunately, however, for Science, Abernethy, recognising the material in the man, obtained for him a post at the College of Surgeons, as assistant to William Clift. From this moment, the brilliancy of Owen's genius commenced to shine, and for fifty years, until age overclouded it, remained undimmed. His first work at the College of Surgeons was to assist Clift in the preparation of Catalogues of the Museum, which had been mainly formed by the anatomist, John Hunter, a man Owen greatly revered, and whose works he later on edited from the manuscripts rescued by Clift from the vandalism of Everard Home.

The publication of a detailed and exhaustive paper on the pearly nautilus (*Nautilus pompilius*), in 1832, at once raised Owen to the foremost rank of working naturalists. Not only was this memoir

complete in a zoological sense, but the views on structure and affinities therein expressed were considerably in advance of his contemporaries.

Continuing the work on the Hunterian specimens after the death of Clift (1849), to whose position as curator he succeeded, Owen issued many special catalogues of the collections, completing the series in 1854. During his twenty years' curatorship of the College of Surgeons, he so rearranged and added to the Hunterian Collections that in 1856 they filled ten times the original space allotted to them, and were suitably housed and displayed in three large galleries specially erected for the purpose.

Owen became public lecturer in comparative anatomy at St. Bartholomew's Hospital in 1834, and in 1836 succeeded Sir Charles Bell as Professor of Anatomy and Physiology in the College of Surgeons. He was also appointed in the same year to be the first "Hunterian Professor" at that institution. He became a Fellow of the Royal Society in 1836, in 1839 was made LL.D. of Cambridge, and in 1843 was elected F.R.C.S. Three years previously he founded the Royal Microscopical Society, and became its first President. In 1844 he was appointed to the Commission of Enquiry on the Health of Towns, and reported on his birthplace, Lancaster. He served also on commissions on the Health of the Metropolis and on Smithfield Market, was president of one of the juries at the Great Exhibition in 1851, and in 1855 filled the same position at the Exposition Universelle, Paris. In 1852, Owen was made D.C.L. of Oxford, and received the same year "Le Prix Cuvier" from the Institute of France, an honour peculiarly acceptable to one who had so industriously and brilliantly followed in the footsteps of that master.

Owen's connection with the College of Surgeons ceased in 1856, when he accepted the Superintendentship of the Natural History Department of the British Museum. Once in this position, he ceaselessly urged the necessity of more space for the growing collections, and in 1859 his "Report, with plans, of a National Museum of Natural History" was ordered by the House of Commons to be printed. Building operations eventually commenced in 1873, and the Museum in Cromwell Road was finally completed in 1880. It is, however, only fair to state that the building as finished but little represents Owen's ideas; one has only to look at his report and plans to recognise his knowledge of what was required, and to see how much the arrangement of the building has been sacrificed to architectural peculiarities. In recognition of his great services to science, Owen received the honour of knighthood on his retirement from the Natural History Museum in 1884.

Richard Owen was a giant, both in stature and in intellect, and the best monument that can be raised to his memory already exists in the Palæontological Galleries of the British Museum. Label

after label calls to mind that Owen first studied and described the remains exhibited, and though he was not free from the errors of the early investigators, and was very jealous of his contemporaries, his triumphs will linger in our memories longer than his weaknesses or mistakes.

Few students realise the magnitude of Owen's work, and it is only those who search in many fields that can comprehend the genius of the man who has now passed to his rest.

C. DAVIES SHERBORN.

II.—SIR RICHARD OWEN'S HYPOTHESES.

IT is now more than sixty years since the Zoological Society published the earliest of those anatomical papers on the Anthropoid Apes, among the later of which was the first thorough and detailed description of the skulls of the Chimpanzee and Gorilla. These, together with skilful restorations of the extinct birds of New Zealand and many other anatomical papers well-known to zoologists, will cause the name of Owen long to live in the grateful memory of all men who have the cause of Natural Science at heart. It seems, indeed, that the fame of our veteran Comparative Anatomist is likely rather to augment than decline. As time goes on and the disputes which formerly arose about the precise definition of "corpus callosum," and the presence of the "hippocampus minor" fade from memory, the many merits of the greatest English Zoologist of the first half of the nineteenth century will, we think, be more and more generally recognised. The esteem in which he has continued to be held during recent years is clearly shown by the award to him by the Council of the Linnean Society of their first Zoological Medal.

However Sir Richard Owen may in some respects have laid himself open to criticism, he has certainly been the victim of some injustice. It may be that a sense of such unfairness had its influence in that award to him of the Linnean Medal to which we have just referred.

This injustice mainly concerns some of his fascinating hypotheses relating to the essential nature of the processes of generation and repair, and concerning certain archetypal principles of our own bodily structure which he first made familiar in England.

We well recollect a brilliant lecture given at the College of Surgeons concerning "virgin reproduction," to which process, so far as we are aware, Professor Owen first applied the term Parthenogenesis. This lecture, revised and enlarged, was subsequently published.[1] It is with respect to this that a great injustice has been committed. It has been, no doubt, unwittingly committed by men who, while lauding or criticising Professor Weismann for his more

[1] On Parthenogenesis. London: John Van Voorst, 1849.

recent contributions to science, have failed to refer to the work and the hypothesis of their aged and illustrious compatriot. Yet in many respects he most certainly anticipated the ideas of the Freiburg Professor,[2] as we have before been careful to point out. It is, however, not so much Owen's hypothesis with respect to virgin reproduction that we desire here to refer to, as to that concerning his ideal generalisations with respect to the structure of vertebrate animals.

At the British Association meeting of 1846, and in his work on the Anatomy of Fishes, published in the same year[3], he put forward views which were fully developed in his work entitled "On the Archetype and Homologies of the Vertebrate Skeleton," published[4] in 1848, and further illustrated in his book, "On the Nature of Limbs,"[4] on the frontispiece of which the human and bovine structures are represented in juxtaposition, much as the skeletons of man and horse have been recently placed in juxtaposition by Sir Richard Owen's most able and indefatigable successor in the directorship of the British Museum of Natural History.

Owen's views were essentially those of Oken, as he lets his readers very plainly know. At page 73 of his work on "The Archetype," speaking of the vertebrate theory of the skull, he says:—"The gifted and deep-thinking naturalist, OKEN, obtained the first clue to this discovery by the idea of the arrangement of the cranial bones of the skull into segments, like the vertebræ of the trunk. He informs us that walking one day in the Hartz forest, he stumbled upon the bleached skull of a deer, picked up the partially dislocated bones, and, contemplating them for a while, the truth flashed across his mind, and he exclaimed, 'It is a vertebral column!'"

Sir Richard Owen's notion was that the skull consists of four vertebræ, with sense organs at the anterior part of each of its four osseous rings. The ear-capsule, in front of the first of the four (the occipital vertebra); the nerve ministering to taste in front of the second, or hinder sphenoidal vertebra; the organ of sight at the front of the third cranial (anterior sphenoidal) vertebra; and that of smell at the anterior part of the fourth or ethmoidal vertebra. He clearly shows (p. 74) how in all this he had been anticipated by Oken, and, on the next page, adds: "This will serve as an example of the close observation of facts, the philosophical association of their relations and analogies, and, in a word, of the spirit in which Oken determines the vertebral relations of the cranial bones. . . . ." Reverting to the petrosal, Oken thus beautifully and clearly enunciates its essential nature and homology:—'You will say I have forgotten the part petrosa. No! it seems not to belong to a vertebra, as such; but to be a 'sense-organ,' in which

[2] In the number of *Nature* of November 14, 1889.

[3] By Longman, Brown, Green and Longman. [4] By John Van Voorst.

[5] Also published by John Van Voorst.

the ear-nerve loses itself; and therefore as distinct an organ from a vertebral element as is any other viscus, or as is the eye-hole itself.' Although Oken does not in this essay formally admit a fourth vertebra anterior to the eye vertebra, he recognises the vertebral structure as being carried out rudimentarily or evanescently by the vomer . . . or the nasal bones."

Now we need hardly say that we do not give this citation with any intention of implying that the views of either Oken or Owen are true; but merely to show that the English naturalist did justice to his German predecessor—even to his suggestions with respect to the vomer and nasals, which are just those which Sir Richard Owen advocated.

In the present day both Oken's and Owen's archetypes have become obsolete, and we suppose no one now maintains them. Nevertheless, when they were first promulgated here, they produced no slight effect, they drew many thoughtful minds towards questions of biology, and they roused an antagonism which has also led to much valuable work. We believe them to have been, in these different ways, very serviceable to science, but we also think that they embodied, or were the mistaken outcome of, some deep and very significant truths which are, in general, far too little appreciated, a wave of sentiment, and the influence of a party (which could do much to make or mar a young man's progress), having combined to indispose many minds towards a dispassionate appreciation of them.

Personally, well disposed as we have always been to Owen, the Hunterian Professor, on account of much courtesy and kindness shown to us by him, we never accepted his representations in any detail, and were quickly convinced that he was quite mistaken as to his "Petrosal" in the lower Vertebrates. Although adopted by not a few persons, some of his views invited hostile criticism, and were soon attacked as being inconsistent, as they obviously were, with the facts of development. It was urged that the skull was made up of modified vertebræ. Its vertebrate character should be plainest in its earliest and least modified stages, and yet such stages had no resemblance to vertebræ at all. It was also pointed out that basilar cartilage and trabeculæ cranii were unlike anything to be found in the incipient vertebral column, the segmentation of which was sufficiently accounted for by the necessary action of pressures and strains on any frequently flexed cylindrical body. A flood of ridicule and sarcasm was poured on Owen's hypothesis, and the doctrine of archetypal ideas was supposed to have received its *coup de grace* from the theory of "Evolution," and above all, from that of "Natural Selection."

But Sir Richard Owen did much more than propound a modified Okenian theory of the skeleton, and indeed it would be a great injustice (an injustice which, nevertheless, has been ruthlessly perpetrated) to represent his theory as *mere* Okenism; for Owen gives no

sign of ever having entertained the fanciful notion that the head was a repetition of the whole body. By the clear and emphatic distinction he made between *Analogy* and *Homology*, by his account of special and general homologues, and by his exposition of serial and lateral homologies, he spread abroad in England the perception that a deep significance underlies the structure of animals—a significance for which no stress and strain, and no influence of heredity, and certainly no mere practical utility, can account.

The temporary overclouding of this perception through the retrograde influence of Darwin's hypothesis of "Natural Selection" is now slowly, but surely, beginning to pass away; for which no small thanks are due to the efforts of his zealous disciples, Professors Weismann and Romanes. It would be out of place to trouble our readers with a restatement[6] of simple facts of lateral and serial homology; we will confine ourselves to once more repeating the assertion that homologies for which neither heredity nor utility will account, reveal themselves in the limbs of chelonians, birds, beasts, and most notably in those of man.

It was, in all probability, a clear perception of this fundamental fact of animal organisation which led Sir Richard Owen at once to reject with disdain the teaching inculcated in the Origin of Species, a teaching eagerly welcomed by that Teutonic "wearer of motley" by whom, as Professor Cunningham well says,[7] *Halophysema* and *Gastrophysema* were "specially created," by the creator of *Bathybius*, and by that prolific Professor at Freiburg whom the destruction by facts of some utterly gratuitous hypotheses, does not for a moment deter from troubling us with others no whit less gratuitous than were their predecessors.

But to return to archetypal ideas and theories as to the nature of the skull. In the first place exception must be taken to embryology as the test. For, if we carry back such researches to the incipient germ, we shall find no available characters for discrimination at all, while diversities of interpretation of nascent structures are almost always possible, and the meaning of initial changes must be elucidated by the study of subsequent changes.

Thus, Professor Huxley tells us,[8] as to *Menobranchus*, that "no definite answer can be given" to the question whether the trabeculæ "grow into adjacent tissues as a tree pushes its roots into the soil," or whether their apparent distinction does not "arise rather from a chondrification of the pre-existing tissue in the immediate neighbourhood of the trabecular cartilage?"

Then, when ossification sets in, the meaning of the several ossific centres, as they arise, must be interpreted by their later stages. How

[6] Readers interested in such questions may be referred to Proc. Zool. Soc., 1884, p. 462.

[7] Natural Science, September, 1892, p. 544.

[8] Proc. Zool. Soc., 1874, p. 199.

else could epiphyses ever be discriminated? Again, the circumstance of a bone or cartilage making its appearance as a single element may in any case be due to the junction of its incipiently distinct parts at a period anterior to possible observation; it may be *connate*, *i.e.*, never distinct to observation, though judged from analogy to be essentially multiple. Of such rationally-inferred but invisible distinctness, we have examples in botany. The *results* of development are surely as much to be considered as are its earlier stages.

As to the vertebral or non-vertebral nature of the skull, it seems to us that such vertebral nature may be affirmed in one sense and denied in another, according to the line of thought which is followed.

It is undeniable that it is within the *laminæ dorsales* of the embryo that the cartilages and bones of both the brain-case and of the dorsal portion of the spinal cord are alike formed. Certainly also the bones of the skull—especially in the higher animals—present a singular reminiscence of vertebræ in the three serially successive arches they form; and if the essence of vertebræ consists in their being a series of bony rings fitted together, and serially enclosing the nervous centres, then, in that sense, the skull is in part composed of three vertebræ. In some fishes (*e.g.*, the sturgeon) the transition from the spinal column to the skull is so gradual that the two are difficult to distinguish. In the Silurian fish *Bagrus* (as Owen pointed out) the vertebræ next the head are greatly expanded, and join each other by suture, and this shows how undoubted vertebræ may simulate osseous cranial walls.

It is true that, unlike the nascent spinal column, the primitive skull presents no serial segmentation, and that the cartilaginous precursors of the cranial bones may even be divided medianly and antero-posteriorly. Nevertheless, in the young axolotl, before the base of the skull has become cartilaginous, an index of transverse segmentation is to be traced in the soft tissue of that region, in spite of the subsequent continuous chondrification of the base of the skull.

But most striking of all is the return made[9] of late years by Sir Richard Owen's main opponent to the conception that serial segmentation, however latent and disguised, extended primitively and fundamentally to quite the anterior end of the head, and that, judging from *Amphioxus*, the skull of the higher vertebrates may be represented as made up of something less than twenty serial segments, each of which is equivalent to a spinal vertebra with its annexed parts.

The conception of cranial vertebræ, then, like the conceptions of serial, lateral, and general homology, are subjective apprehensions of relations having an objective existence in nature. They are like conception "types," which are not, of course, real objective entities, *as types*, though they are conceptions which have none the less an objective basis. The acceptance of the theory of evolution is no bar to the reception of the view which represents all organic forms

[9] Proc. Roy. Soc., no. 157, p. 127.

as having been specially created according to certain fixed and pre-ordained ideal types. The two beliefs, far from being reciprocally exclusive, can and do exist in perfect harmony.

Immediately underlying the question here touched upon reposes one of the deepest and most important of all philosophical questions, but we have no space to enter upon its consideration here. Profoundly impressed as we are, however, with the significance of questions touching philosophical anatomy, we cannot refrain from expressing our sense of the deep gratitude the world owes to Sir Richard Owen, who, years ago, forced questions such as these upon willing and unwilling ears. The details of his system are erroneous, much of it is fanciful and baseless. But he has nevertheless enriched zoology with a knowledge of a multitude of valuable facts, and many of his memoirs are models for those who come after him. To not a few kindnesses, and to many worthy actions, unknown to the world, we can testify. Of him, at least, it can never be said that he sought eminence through the degradation of his species, or that with selfish recklessness he threw broadcast seeds producing fruits fatally poisonous to all that is highest and best in man, or that gives any virtue, beauty, and dignity to human life. His work is done, his troubles, his labours, and disappointments are over. May his end bring him peace.

St. George Mivart.

III.—SIR RICHARD OWEN'S RESEARCHES ON THE INVERTEBRATA.

In the course of more than half a century's active devotion to scientific research, Sir Richard Owen has investigated the structure of almost every group of living and fossil organisms from man to the internal parasites affecting his frame. His contributions to knowledge of the invertebrated animals are not so numerous as his publications on the living and extinct forms of the vertebrata, but his attention was nevertheless directed successfully in early life to this interesting division of the animal kingdom, and his original memoir on the Pearly Nautilus, published in 1832, when he was only twenty-seven years of age, laid the foundation of his well-deserved fame as a physiological anatomist. Just twelve years later he was awarded the Copley Medal of the Royal Society, in recognition of the merits of a memoir on the fossil "Belemnites" published in the Philosophical Transactions.[10]

The general results of Professor Owen's investigations of the Invertebrata were epitomised in his Catalogues of the Fossil Invertebrata in the Museum of the Royal College of Surgeons, in which more than three hundred and fifty species of fossil cephalopoda collected in the previous century by his famous predecessor in office, John Hunter,

[10] In conjunction with the Memoir on the Monotreme Mammalia.

were described; in Lectures on the Comparative Anatomy of the Invertebrata, largely based on discourses delivered during his occupancy of the "Hunterian Chair," in that great institution, the foster-parent of so many distinguished reputations; in an article on Cephalopoda contributed to the "Encyclopædia of Anatomy and Physiology," and in that on the Mollusca, including the Molluscoidea, to the eighth edition of the "Encyclopedia Britannica." A remarkable article, entitled "Palæontology" was first published in the latter work, and subsequently enlarged and re-issued as a "Systematic Summary of Extinct Animals and their Geological Relations" in 1861 as a separate publication. (Part I., relating to Invertebrata.) This is one of the best examples of Professor Owen's literary powers of popular exposition of technical details. The Memoir on "Parthenogenesis," treating of the phenomena of asexual generation among certain groups of Invertebrates, belongs more properly to the category of his speculative and philosophical inquiries.

We will now refer briefly to some of the most important of Professor Owen's publications on invertebrates, grouping them for convenience more in zoological order than chronological sequence. Observations on the structure of a new genus of "well-woven" sponge *Euplectella aspergillum*, Owen, appeared in 1841; and in 1857 a new species of the same interesting genus was described. "The threads of the *Euplectella* were not first spun and then interwoven," he wrote in his second memoir on the genus, "but were formed as interwoven, the two processes going on simultaneously; or *pari passu*, in the living state, the exquisite structure of the flinty framework may be veiled by the delicate, gelatinous, enveloping, organic tissue."

No less than five memoirs on the Entozoa appeared in the first volume of the Transactions of the Zoological Society issued in 1836, and his comprehension of "the structural differences existing among them" induced him to propose a more natural classification of this difficult group. His descriptions and figures of a new endo-parasite, first discovered by Mr. (now Sir) James Paget, his fellow student, at St. Bartholomew's Hospital, infesting the voluntary muscles of a human subject, and the subsequent recognition by Zenkrer of *Trichina spiralis*, Owen, as the cause of a troublesome and often fatal disease now known as Trichinosis, led to results of practical utility to mankind.

An interesting paper, entitled *Protichnites*, was published in 1852, wherein the fossil footprints of ancient crustaceans were tracked in the sands of time. "Old Nature speaks as plainly as she can do by these distinct symbols, and if we do not fully thereby read her meaning, the fault is in our powers of interpretation. The creatures which have left their impressions on the most ancient of known sea shores belonged to an articulate and probably crustaceous genus, and *Limulus* comes the nearest to my idea of the kind of animal which has left the impressions on the Potsdam Sandstones."

Twenty-one years later Professor Owen published an elaborate and well-illustrated memoir on that interesting, abnormal, but mis-named arthropod, the King-Crab (*Limulus polyphœmus*), which is no crab, and has since been definitely promoted out of the class of water-breathing Crustacea into that of the air-breathing Arachnida and associated with the Spiders and Scorpions.[11] He detailed the muscular, nervous, digestive, and generative systems, and proposed the term "Cephaletron" for the anterior division of the body, and "Thorocetron" for the second; while he declared "the chief fossorial agent, as indicated by the size and disposition of the principal muscular masses, to be the cephaletral digging-shield." While recognising some of the Arachnid affinities of the King-Crab as then maintained by Strauss-Durckeim and Latreille, Owen did not admit the necessity for removing the genus from the order of Merostomatous Crustaceans.

Professor Owen's numerous memoirs on the anatomy of the Brachiopoda were second only in importance to his researches on the Cephalopoda. In both classes he founded orders which have been generally[12] accepted by biologists, and still maintain their position as recognised additions to zoological nomenclature and classification. In the article on "Mollusca," published in the eighth edition of the "Encyclopedia Britannica," the sub-kingdom Mollusca was divided into the *Heterogangliata*, with one nerve ganglion, and the superior *Homogangliata* of Owen. The group was compared to a tree, with the Cephalopoda at the summit, one of the roots appeared to be lost in the Turbellarian and the Trematode families of Abranchiate Vermes; the Brachiopoda conducted to the Bryozoa, and both, with the Tunicata, were considered as "mollusc-like" rather than molluscan. The Brachiopoda were regarded as a class equivalent in value to the Lamellibranchiata, and were placed between those acephalous molluscs and the sea-squirts, that is to say, between the "Acéphales testacées," and the "Acéphales sans coquilles" of Cuvier. In 1833 the anatomy of the "Cuvierian genera of Brachiopoda," more especially of *Terebratella* and *Orbicula* (now known as *Discina*), were described. The circulatory, muscular, and nervous systems of other genera were more fully investigated twenty years later, and the results published, accompanied by beautiful drawings by the author, in a "Memoir on the Anatomy of Terebratula," in the Introduction to Davidson's classical Monograph of the "British Fossil Brachiopoda," elucidating some points in the structure of *Waldheimia* and *Lingula*. Owen's brachiopodal hearts are now known

[11] "Limulus an Arachnid," by E. Ray Lankester, F.R.S. *Quart. Journ. Micros. Sci.*, 1881.

[12] Waagen and F. A. Bather excepted. Mr. A. H. Foord, in his exhaustive "Catalogue of the Fossil Cephalopoda in the British Museum," 1888 (p. viii., Int., part i., Order Nautiloidea), states that "no evidence has as yet been brought forward that the fossil forms included by Owen in the Tetrabranchiata were other than four-gilled."

to be oviducts, and it has since been made evident that he did not recognise the closure of the intestine in certain genera—a physiological feature of ordinal value. But he divided the class into two orders, so well differentiated by other characters, that the names of *Lyopomata* (or loose valves) and *Arthropomata* (or jointed valves), which he applied to them respectively, should retain priority over the *Articulata* and *Inarticulata*, the *Tretenterata* and *Clistenterata* of other investigators, of which subdivisions they are the exact equivalents. Owen's researches on the internal structure of the Brachiopoda marked a distinct advance in knowledge of this difficult and obscure group of organisms.

In a memoir on the anatomy of *Clavagella*, Professor Owen noted the enormous development of the mantle in the "club shells" as one of the instruments in the work of excavation for which these bivalves are remarkable. Some observations on the camerated structure in the valves of the water-clam (*Spondylus varius*) were first printed in the *Proceedings of the Zoological Society* for 1837, when attention was directed to the chambered and septated condition of the shell in that genus, and to the connection existing between the mode of growth in *Nautilus* and that of other molluscs which partition off a part of their shell not required for the convenience of the soft parts of the animal inhabiting it. It was then distinctly stated that the septal divisions of the *Nautilus* shell are analogous to the septa of the "water-spondylus," and many other shells which partition off the disused portion of their houses both among univalves and bivalves. An investigation of the anatomy of the gasteropod family of the "bonnet-shells" (Calyptreidæ) proved of interest "as manifesting some of the successive stages of complexity in the passage from the simple *Patella* to the spiral valves." Professor Owen's studies of the univalve mollusca in general, and of the recent *Nautilus* in particular, led him to the conclusion that the Pteropoda had less affinities with the Dibranchiate, or "two-gilled" cephalopoda, than the recent *Nautilus*. This form he considered to be the type of an inferior order of tetrabranchiate, or "four-gilled" cephalopoda, represented by numerous fossil species, and as an osculant form between the cephalopoda and gasteropoda. In consequence of this conviction, he transferred the encephalous floating Pteropoda from above to below the Gasteropoda[13] in his classification of the Mollusca, which is based chiefly on the conditions of the respiratory organs, as a physiological character intimately connected with activity of locomotive functions.

The subject of the Pearly Nautilus had been strongly recommended to his attention by Baron Cuvier, in whose dissecting rooms young Owen enjoyed the privilege of working. The famous French zoologist had never seen a specimen of the animal of this genus,

[13] The Pteropoda are now considered to belong to the inferior branch of the class Cephalopoda by those who place the class Gasteropoda at the summit of the Molluscan series.

which was known to Aristotle, and had been briefly described by Rumphius. It was, therefore, with peculiar satisfaction that Mr. Owen received an example in alcohol which had been captured, in a dying condition, by his friend and former fellow-student at the College of Surgeons, Dr. George Bennett of Sydney, while cruising in the Polynesian seas. The results of his investigations appeared in the "Memoir on the Pearly Nautilus, with illustrations of its external form and internal structure," which, to the keen regret of the author, was not issued from the press until three days after the death of his illustrious master. The animal was shown to be characterised by the presence of pedunculated eyes, calcareous mandibles, like a bird's beak reversed, a crop, gizzard, and liver, four gills, or breathing organs, and by the absence of a branchial heart and ink-bag. It occupied the last and largest chamber of a nacro-porcellanous, many-chambered shell, into the last cell of which it could retract itself and its numerous simple non-aceptabulated arms closing the mouth of the shell by the dorsal arms and by a horny hood-shaped structure, the analogue of the aptychi or double operculum of the fossil Ammonite. A central siphuncular tube pierced all the septa running through all the chambers or so-called "air-cells" which had been occupied by the animal in succession. This siphuncle "subserved a hydrostatic function," enabling the animal to rise to the surface and sink to the bottom, possibly by means of a gas secreted from the mantle. But the author's remark that "much remains to be done before the theory of the chambers and siphuncle can rest on the sound basis of experiment and observation" is still applicable.[14] The order Tetrabranchiata was proposed to receive the recent *Nautilus* characterised by the presence of four gills, a many-chambered external shell, and the absence of an ink-bag; and the fossil Ammonites and their allies were included therein. The Dibranchiata comprised all the two-gilled naked Cephalopods with an internal shell, or pen, and an ink-bag such as the living octopoda, the squids, cuttles, and the fossil Belemnitidæ.

In 1844 Professor Owen resumed the study of fossil cephalopoda, and communicated descriptions and figures of "certain Belemnites preserved with a great proportion of their soft parts in the Oxford Clay at Christian Malford, Wilts." The guard sheath or rostrum, known as the dart or javelin, whence the name Belemnite was originally applied, was described; the chambered or vertically septated portion was named the phragmacone; while the presence of an ink-bag, internal shell, muscular mantle, and aceptabulated arms were duly noted, and the fossil molluscs were classed with the recent

[14] There appears to be no doubt that the deserted chambers of the *Nautilus*-shell contain, in the healthy living animal, a gas which serves to lessen the specific gravity of the whole organism . . . . "A certain stage is reached when no new chamber is formed; with regard to its origin we can only conjecture; the whole matter is involved in obscurity." Ray Lankester, ninth ed., Ency. Brit., 1883.

Spirula, in the order Dibranchiata. The examples of *Belemnites oweni* upon which this memoir was founded, were subsequently shown by later investigators to belong to the animal of *Belemnoteuthis*, an extinct member of the existing squid family, and the animal of the true Belemnite proved to be characterised by the presence of an internal pen or pro-ostracum not found associated with the fossil remains of *Belemnoteuthis*.

In the same year Professor Owen communicated to the Zoological section of the British Association two papers detailing the "Further Experiments and Observations on the *Argonauto argo*," by Madame Jeannette Power, the originator of marine aquaria. He prefaced them with some remarks on the relation of the animal of the Paper Nautilus to its shell. The experiments which proved that the dorsal arms were the fabricants of the shell instead of the mantle, as in the Pearly Nautilus, had been suggested to Madame Power by Professor Owen. He has also described *Rossia*, a sub-genus of *Sepiola* (*R. palpebrosa*, Owen). In 1848 he enjoyed an opportunity of dissecting an unique but fragmentary specimen of the animal of *Spirula peronii*, and a portion of *Spirula reticulata*, and contributed the results to the "Zoology" of the "Voyage of H.M.S. 'Samarang.'" Thirty years later he returned to the subject of the structure of this interesting genus.

In 1878 Professor Owen discussed with much vigour "The Relative Positions to their Constructors of the Chambered Shells of Cephalopods." In this paper he showed that the shells of *Nautilus* and ammonites are revolutely spiral, or coiled over the back of the animal, not involute like the *Spirula*, and maintained the opercular character of the aptychi, which had been doubted by Keferstein and Waagen. He reiterated the opinion that they correspond to the fibrous hood of *Nautilus pompilius*, and are the calcifications of an ammonite hood. The fact that no trace of the ink-bag has ever been found with any fossil ammonite although that organ occurs abundantly in the fossil Belemnitidæ, is cited as conclusive evidence that the Ammonites were tetrabranchiates protected by an external shell, and he points out that the occurrence of the protective ink-bag in the *Spirula* proves that mollusc to belong to the more active dibranchiate order, with an internal chambered shell—the homologue of the phragmacone of the Belemnite.

The ink-bag does not occupy the last chamber of *Spirula*, as had been maintained in Woodward's "Manual of the Mollusca." The situation of the small pyriform ink-bag in that genus being "rectal," as Owen first demonstrated in 1848. It is denied that the septated condition of the many-chambered shells of the *Nautilus* was related conditionally to the generative function and periodical increase, as had been asserted by Professor Seeley and by Dr. Henry Woodward, for the reason that the formation of such chambers commences from the embryonal cup ("protoconch") and continues through an early

period of growth antecedent to the acquisition of the procreative functions, or the adult stage of existence, and that these early chambers are relatively deeper than the succeeding ones.

In this comprehensive review of the subject Professor Owen states that he "holds by the opinion expressed in his original memoir and in the 'Catalogue of the Fossil Cephalopoda in the Hunterian Museum' (4th ed., 1856, p. 29), that they so affect the specific gravity of the active, highly-organised, cephalopodous mollusc, as to enable it with little èffort to rise, in the case of the *Nautilus*, from its habitual position at the bottom of the sea, and in the case of the *Spirula*, to sink from its more usual zone at or near the surface, by means of the hydrostatic mechanism worked by the muscular forces of the mantle and funnel. The constancy of the siphuncular connection running through all the chambers of the largest and most complex of the polythalamous shells, with the great size and singular complexity of the siphuncle in several extinct species, form the grounds on which I still hold to my original belief in the function of the siphuncle as related to a maintenance of the vitality of the shell."

In the following year, Professor Owen published the results of his dissections of a perfect specimen of *Spirula australis* received from Mr. Cuming. The animal was described as almost as devoid of external organs of natation as *Nautilus*. In both, the direction in which such forces act is retrograde. *Nautilus* exercises them mainly by virtue of the muscular funnel, through which it forcibly ejects into the surrounding water the respiratory current. *Spirula* superadds to this the ejection of that volume of water upon which the cephalic arms and their basal webs contract after the fashion in which the other Dibranchiata, especially the Octopods, propel themselves backwards. *Spirula* is superior to *Nautilus* in the cephalic mechanism. In both instances of multilocular cephalopods, the natatory power is inferior to that of existing Dibranchiata. The distinction, therefore, between *Nautilus* and *Spirula* in regard to the shell in its protective relation is relative not absolute: in the one a small proportion of the shell is occasionally "internal," in the other a small proportion is always "external," in both the multilocular shells correspond with the phragmacone of the Belemnite. The tetrabranchiate *Orthoceras* may be called a representative analogue of the dibranchiate Belemnite, as the tetrabranchiate Ammonite is of the dibranchiate *Spirula*. The siphon is "ventral" and marginal in both kinds of coiled shells, but it runs along opposite sides of the coil. In *Spirula*, its position is internal, or ento-marginal; in ammonites, it is external or "ecto-marginal."

In 1883 the veteran anatomist published a paper on the "Aspect of the body in Vertebrates and Invertebrates," which formed, we believe, his last contribution to the anatomy of the invertebrated animals. His researches cover a period of fifty years and afford

constant proof of the patience, industry, and acumen of the author. "The progress of Palæontology since 1830," he wrote in 1866, "has brought to light many missing links unknown to the founder of the science. My own share in the labour led me, after a few years of research, to discern what I believed, and still hold to be, a tendency to a more generalised or less specialised organisation as species recede in date of existence from the present time."

AGNES CRANE.

(To be continued next month by an account of Sir Richard Owen's Researches on the Vertebrata.)

II.

Artificial Protoplasm.[1]

LET it be said at once, for the benefit of the timid, that Professor Bütschli does not claim to have made protoplasm. His sumptuous quarto contains the results of ten years' special work on the physical side of the phenomena of life. The author is very strongly opposed to that school of Biologists who attempt to surround the phenomena of life with unnecessary mystery, and who clamour at once timidly and virulently against all physical and chemical interpretations. He has now been able to make microscopic foams which in structure and behaviour, under various conditions, marvellously resemble the structure and behaviour of protoplasm. These foams have enabled him to bring together the observations of himself and others on the actual structure of protoplasm, and to furnish a striking theory of its physical nature.

He tells us that he began his attempts at the making of foams with the forlorn feelings of a nineteenth century alchemist. The starting point was this: If fine particles of a substance soluble in water be mixed with a fatty oil, and the mixture be placed in water, the water diffusing into the oil becomes arranged in minute droplets round the particles of soluble material, and a foam is formed. Owing to the difficulty of breaking up the soluble substance into sufficiently minute particles, the particles, viewed under the microscope, are very coarse. The drops are placed on the under side of a coverslip with wax feet, and the coverslip is placed in a drop of water on a slide. When such a slide is put in a damp chamber for about 24 hours, the drop becomes quite milky and opaque. When it is flattened out by pressing on the coverslip, or made transparent by allowing glycerine to diffuse through it, this drop shows that it is composed of a very fine froth. It seems probable that the froth is caused by the presence in the oil of a small quantity of soap. The soap is more soluble in water, and the solution of soap and water has gradually separated out in very fine droplets. Bütschli empirically found the best way to produce these froths. Fresh olive

[1] Untersuchungen ueber Mikroskopische Schaeume und das Protoplasma. By O. Bütschli. (With 6 lithographic tables, and 23 figures in the text.) Leipzig: Engelmann, 1892. Price 24 marks.

oil must be kept in a watch glass, at a temperature of from 50–60° Centigrade, for twelve days, or at 80° C. for about three days. It becomes nearly colourless, and rather thicker. This is placed in a mortar, and powdered, but not anhydrous, carbonate of potash is rubbed into it. Although the desired result is the formation of a soap in the oil, good froths are not obtained by adding fatty acids or soaps to the oil.

The prepared froths, then, consist of a multitude of tiny droplets of a solution of soap in water, each droplet being surrounded by a film of liquid oil. The films, when they are in contact, naturally run together, and so a structure of polyhedral figures is built up, just as when one blows into soap solution in a saucer, the mass of bubbles can be seen to consist of polygonal figures. Minute examination of the froth-structure shows that at the nodes where the films meet, owing to their liquid condition, slight expansions occur (*op. cit.*, p. 20, Fig. 2). These expansions, when the bubbles are very small, give a granular appearance to the whole structure—an appearance which is more striking when a part of the froth more than one layer of bubbles thick is examined. The general optical effect is that of a meshwork, with thickenings at

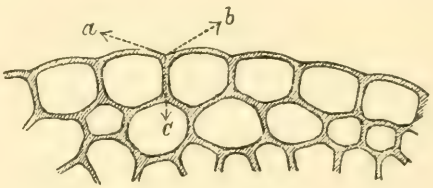

FIG. 1.—Foam under high magnification showing reticulate and fibrillar appearance and nodes.

the nodes. Bütschli found that when adventitious particles as, for instance, of lamp-black, are mixed with froth, these particles tend to collect at the nodes; but a careful study of the froth shows that node thickenings alone are enough to account for the granular appearance of protoplasm. He also shows how the familiar fibrillar, reticulate, and lamellar appearances, on careful focussing, are resolved into arrangements of bubbles.

He shows and explains, theoretically, how surface tensions make drops of such a froth, suspended in a liquid, assume a globular form, save under the influence of special internal or external forces. For some not very clear reason, the larger bubbles come to group themselves in the interior of each drop. Round the periphery, where the surface tensions are similar, a single row of bubbles of equal size becomes arranged (Fig. 1). The films separating these bubbles are radially arranged, and so there is formed a definite alveolar border. When the bubbles are small, this looks like a striated border. A similar border is naturally formed round any internal large vacuole. The alveolar layer, spite its definite appearance, remains as liquid as the rest of the foam, and persists while the drop is flowing.

These drops show very remarkable streaming movements. While they are in a weak solution of carbonate of potash they remain at rest. But this solution may be removed by drawing pure water under the coverslip on its wax feet by means of pieces of filter paper. Thereupon the drops, if they are not pressed on by the coverslip, move about actively. During this movement they may change in shape, the change consisting in the extrusion of processes of the foam in the direction of the movement. The internal streaming movements can be seen when the opaque drops are cleared by glycerine. It consists of an axial stream from the centre of the drop to the edge in the direction of the movement through the liquid of the whole foam. At the edge the stream turns back over the surface of the drop, and the backward streams so formed join the axial stream posteriorly. All this corresponds very closely with the streams and motion of a simple amœba. After a time the streaming from one centre ceases, a new set of currents form from a new centre,

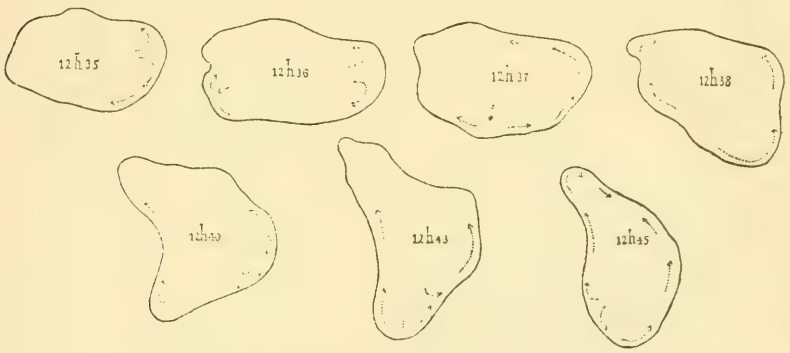

FIG. 2.—Figures of streaming movements observed in a drop.

and movement in a different direction occurs. Sometimes several centres of streaming are present together, and the whole drop excessively resembles an amœba. The streaming goes on from 24 hours to at most six days (Fig. 2).

The theoretical explanation of these movements depends on surface tensions. When a liquid is brought towards a drop of a second liquid suspended in a third, if the surface tension is less between the first and second than between the second and third, as the first liquid approaches the second the drop of the second stretches out towards the first, and a set of streaming movements occurs. Thus, in the figure some soap solution (first liquid) is brought up towards a drop of oil (second liquid) suspended in water (third liquid). Then the drop of oil moves gradually into the soap solution, and the currents shown by arrows in the figure appear (Fig. 3). The surface tension between the oil and soap solution is less than the surface tension between the oil and water. The equilibrium is destroyed and the soap solution spreads round on the drop from the

point of contact. As it passes round it gets more and more diluted, and the equilibrium continues to be disturbed. The surrounding fluid presses in to replace the soap solution, which is carried backwards from the point of contact, and this general pressure, acting alike on oil, soap, and water, moves up the oil drop slowly towards the point of contact with the soap solution.

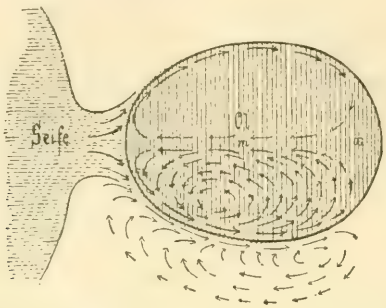

FIG. 3.—Soap solution (*seife*) in contact with drop of oil (*ol*) suspended in water, showing streaming movements.

In the actual case of the froths, the soap solution is contained in the cells of which the froth is composed. Centres of streaming movements are produced by the soap solution diffusing out into the surrounding liquid or by the bursting of superficial cells.

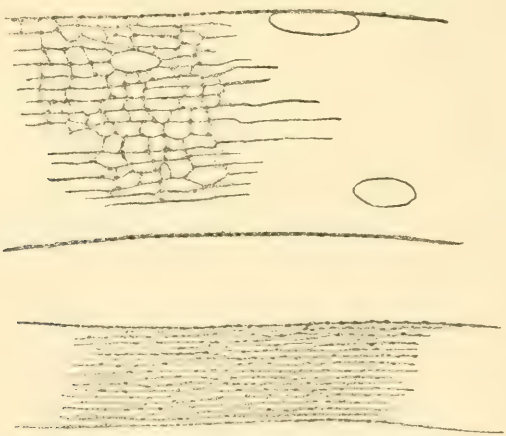

FIG. 4.—Foam of olive oil and carbonate of potassium, very viscous, much magnified, and pressed into fibrils by the cover glass. The upper figure is a higher magnification of part of the lower figure.

From a consideration of these froths Professor Bütschli passes to the observed appearances of actual protoplasm in the animal and vegetable kingdom, and from a large number of instances collected from all manner of cells, he shows how readily these appearances can be interpreted if we suppose protoplasm to have a frothy structure like the structure of his artificial drops. Of these two instances are reproduced opposite.

A large part of the remainder of the book is occupied by a discussion of various existing theories of the structure of protoplasm, and it concludes by foreshadowing possible explanations of various phenomena like the streaming movements in plant cells, and muscular contraction, upon the basis of his interpretation of the structure of protoplasm.

Put generally, Professor Bütschli's results are these: Protoplasm is itself a special chemical substance, the seat of many chemical changes. But it is also a physical substance, with definite physical texture, and obeying definite physical laws. It is possible to construct foams of different chemical nature but with similar physical properties. These foams show that protoplasm is composed not of a network containing sap and granules, but of a series of delicate bubbles obeying the physical conditions that rule bubble structures. The contents of the bubbles are some kind of fluid—a fluid which can be replaced by glycerine and other reagents. Many of the familiar features in

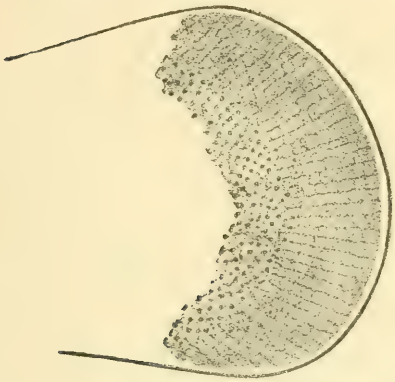

FIG. 5.—Protoplasm of *Amœba* (*Cochliopodium*?) *actinophora.*

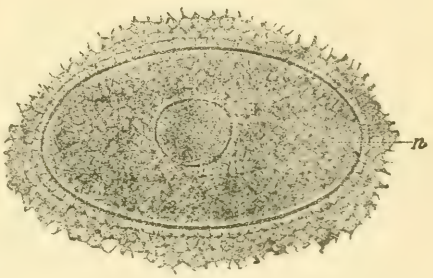

FIG. 6.—Nucleus with part of surrounding plasma from gangliar cell of ox.

the microscopic appearance of protoplasm—features like the network, the granular and fibrillar appearances, the striated alveolar layers, are the optical expression of this frothy structure.

Streaming movements, changes of shape, progression through fluids, are physical conditions. In artificial froths they can be referred easily to differences in surface tension inside the froth and between the froth and the surrounding fluid. The more complex chemical conditions and the active chemical changes in protoplasm render it highly probable that complicated changes of tensions within and without the cell are constantly being produced, and so there is to be expected a more lifely manifestation of the movements of streaming, of change of position and change of shape, which are actually present in the relatively simple artificial froths. A combination of fuller knowledge of the chemistry of protoplasm, combined with this fuller knowledge of its physical structure, would go far, if not the whole way, to an understanding of life.

This must suffice for a short account of the nature of Professor Bütschli's investigations. I am glad to be able to say that my friend Mr. E. A. Minchin has arranged with the author and publisher for the production of an English translation of this work. Until the appearance of this, everyone who can spare time to cope with an intricate and rather perplexing German style should go to the original. Vitalism, the empirical study of living organisms as living, may be the most presently fertile working hypothesis. But only those of little hope look for no final synthesis of the facts of Biology, Physics, and Chemistry, and even now they will have much ado when they try to discount Professor Bütschli's brilliant work.

P. CHALMERS MITCHELL.

IV.

Phases of Evolution in the Guiana Forest.

THE great forest region of South America, which extends almost to the shores of British Guiana, is one of the most interesting in the world to the naturalist. An ordinary observer may, perhaps, think its dark arches tame and dull, but even he is delighted with the beauty and variety conspicuous at every bend of a creek or on the edges of the savannah. Unlike the woods of Europe, so uniform in character, we have here trees of a hundred species struggling with each other for position, or straining their utmost to hold their own in the great battle for life. It is hard to find two individuals of the same species growing together; each appears to be fighting for himself, regardless of the others, whether akin or not.

As a result of this, specialisation is a most prominent characteristic, not only of genera and species, but even of individuals. The oaks and pines of temperate climates are undoubtedly variable to a considerable extent, but will not compare with the great family of Leguminosæ in the tropical forest, where a thousand contrivances are manifest in trunk, leaf, flower, and fruit. The different forms of the legumen alone are particularly interesting. Here we have the trysil (*Pentaclethra filamentosa*) with pods a foot long, and, farther on, the wallaba (*Eperua falcata*), which hangs its scimitar-shaped fruit on long strings. Then the tonka-bean with plum-like fruit, the arisaro and others with winged seed-vessels, and a host of shrubs and climbers with pods so curious and wonderful as to baffle the naturalist when he attempts to find reasons for their existence. Other natural orders, such as the Lecythideæ and Lauraceæ are also particularly interesting.

At first the wealth of species is almost bewildering; the forest alone would give work for a lifetime. It is so grand and magnificent —so crowded with immense trees, bush ropes, epiphytes, and parasites—so varied in species, as first to excite enthusiasm and then depress with the thought that life is too short to investigate it thoroughly.

Coming from a temperate climate, where the number of species in a wood can be counted on his fingers, the European sees above him a canopy of intermingled branches and foliage of a hundred different kinds which he cannot even reach, much less identify.

Nothing but an interminable assemblage of columns and a green roof above is seen, and even these but indistinctly, as not a ray of sunlight can penetrate the canopy.

On the ground the soil is covered with fallen leaves in various stages of decomposition, some already sunk into the rich brown humus so characteristic of the forest, others still retaining their shapes, and a few, newly fallen, mingled with petals and perhaps monkey-pots, pods, or other fruits. In one place the ground is covered with petals and the botanist turns to look for the tree from whence they have fallen, or even takes out his glass—but all in vain, for no particular tree can be separated from the throng; the flowers are outside in the light, and quite invisible from below.

Only on the edge of the forest, where it borders on the river or savannah, can the trees be identified. Here a mass of yellow shows that an etabally (*Vochysia*) or some leguminous tree is flowering, but it is almost impossible to procure a specimen. When the tree rises sixty or eighty feet without a branch, nothing less than cutting it down will serve. A few broken spikes may be brought down by a shot, or, perhaps, if the traveller is fortunate, one of his Indian boatmen may cut off a branch with an arrow, but to do either he must be fortunate indeed. True, many a specimen may be gathered alongside the river or creek, but some of the great forest giants are not found in such a situation. Even the mora (*Mora excelsa*), which is so common, hangs its flower spikes far overhead in a most tantalising manner, as if defying the botanist to gather them. With so few outside enemies, the trees of the forest have only to contend with each other for their shares of the light and moisture. Hardly a single ray can reach the ground. On the banks of the rivers where the continuity is broken, hosts of epiphytes crowd the great limbs and branches, utilising the diffused light. Seated on the upper sides, they extend outwards and downwards until they almost form curtains. Even those which grow upright often push their flower-spikes from the side or base so that they may gather more light away from the crowd. Ferns and orchids have developed this faculty to its greatest extent, hanging their long leaves straight downwards until they sometimes touch the points of other plants on the branch below.

If these do not take up every ray, a jungle of palms, tree-ferns, marantas, and arums crowd together underneath, along the river banks, and prevent all access to the forest except by cutting a way through them. If the river-current is strong, no water plants can secure a footing, but in every creek there are bays where, given a little sunlight, patches of *Cabomba aquatica*, or water lilies, manage to exist.

The atmosphere inside the forest is hot, dense, and steamy. To the old colonist it suggests intermittent fever, but to the naturalist its peculiar odour recalls many a pleasant journey through creek and wallaba swamp, or, perhaps, remembrances of anxious hours when

he found himself lost in the forest. The perspiration streams from the new-comer, and unless he is enthusiastic in pursuit of his hobby he is glad to find himself again on the river. Where the land is low, pools of ruddy-brown water occupy the hollows between the immense buttresses of every tree, while here and there small streams wind in and out among them. The fallen leaves and other *débris* form an oozy kind of peat, obviously giving colour to the water, which is the same in almost every creek and river in Guiana.

It is generally considered that the forest is unhealthy for man. Residents along river banks suffer more or less from intermittent fevers, which often leave them after a change to the coast. In opening up new land by clearing, it is notorious that strangers suffer much while the work is going on. Swamps are also unhealthy for strange plants. Few cultivated fruit trees can endure inundations for even a day or two. Yet, under these conditions, the trees of the forest thrive. They have obviously become accustomed to environment such as would be dangerous to nearly all others. Is it not possible that what is dangerous to a plant is just as injurious to man, unless he has protected himself in some way? Also that there may be lessons to learn from the way trees have warded off the danger?

It is an undoubted fact that the forest giants of Guiana have evolved contrivances to prevent the development of low fungi. The most common of these is a thick bark containing tannin. This tannin is Nature's great antiseptic. It is found in all the waters of the forest and swamp, and accounts for that great difference which exists between the clear brown bog water and that seat of infection the stagnant pool. In towns and villages ordinary rain-water, if allowed to remain in open trenches, never resembles in its antiseptic properties that of the forest. The Indian has gone to the forest and learnt a lesson. When suffering from ulcers he scrapes the inner bark of some tree and applies it to the sore with most beneficial results.

Besides this, many other secretions have been developed. One of the most extraordinary is, perhaps, the milky juice which dries into india-rubber and gutta-percha. It is well-known that the Hevea grows only in places that are periodically inundated, and this characteristic is also strikingly exemplified in the tree which produces balatta, the gutta-percha of Guiana. It may be objected that some of the species of *Ficus*, which have similar juices, do not necessarily belong to the swamp, but perhaps their ancestors lived under different conditions. These secretions are so evidently fitted for protection against water that we can hardly conceive of them in any other connection.

Then there is another large class, the gum-resins and oleo-resins, also common in Guiana. Gum animi, the incense gum from the hyawa *(Icica)*, and balsam copaiba are the best known examples. The species of *Copaifera* are characterised by their thick bark, from which the Indian wood-skin canoes are made, and which are preserved

for a long period by their impregnation with the balsam. The tree which, perhaps, above all others delights in the inundations of the forest is the wallaba. In this we have not only a secretion of tannin, but also an oleo-resin which impregnates the tree to its very heart. Others have similar balsams, while some are so impregnated with essential oils as to smell quite strong, although not always pleasant. These secretions make the Guiana timbers so valuable, as they endure longer under water than those from any other part of the world.

Bitter extractives are found in the barks and seeds of many trees. Simaruba and greenheart are well-known, as they may be found in the British Pharmacopœa. No doubt they are also protective, perhaps chemically. Is it not possible that there is some truth in the old belief that medicines grow in every country suited to the disorders endemic to the place? The true explanation of this would be that plants protect themselves against evils which also affect man, and he, going to nature for a lesson, learns to follow their example.

Protected against floods and excessive dampness by so many contrivances, the forest trees are able to secure moisture at all times without suffering from drought or flood. With such a wealth of rotting leaves about their roots, there is never any lack of plant food, and as for light, they have to struggle hard for that. In doing this we cannot help seeing that they are intensely selfish. We can hardly conceive of selfishness without a self—an individual—and that every tree, and even the humblest plant, is such the naturalist is bound to admit. No matter that he cannot recognise one from another except in but few instances, the individuality is there and has been working for ages to raise the species to its present stage. According to the amount of individuality so will progress be greater or less. Heredity and environment may account for a great deal, but there is always something more than inherited traits and force of circumstances. Evolutionists have apparently thought too much of species and too little of individuals. At some time or other the individual was father to the species, and a few of the present generation may be progenitors of new forms, the careful observation of which will give the naturalist of the future the strongest proofs of evolution.

The tropical forest is undoubtedly made up of individuals. There is little of that apparent unison so common in temperate climates, where the trees open their leaf- and flower-buds together and at regular seasons. Here the ordinary observer says there are no seasons, but that trees flower and fruit all the year round. This, however, is an exaggerated statement not borne out by facts. There are two cycles in a year, the first in British Guiana commencing in February, and the second in August. As might naturally be expected, some species and varieties are earlier or later, and not only is this the case, but hardly two individuals flower, fruit, or drop their leaves at exactly the same time. This is most strikingly exemplified in the mango and other cultivated fruit trees. At the

present time (September) the mango has been flowering for two months, and trees may be seen in different stages from unopened flower buds to half-grown fruit. The fruit will ripen from December to February, when the great mango-season will be over, and only a fruit here and there be obtainable until June and July, when a second crop matures. The result of the two cycles and the difference in flowering and fruiting of individual trees is that most tropical fruits are obtainable more or less for six to eight months of the year, being, however, far more plentiful during the few weeks of each season when the majority ripen.

It is hardly necessary to bring evidence in favour of individuality among cultivated plants. Every gardener and florist knows that it is impossible to predict absolute uniformity in the offspring of a single plant, even when the greatest care is taken to prevent crossing. Everything goes to prove that the same individuality can be found, more or less, throughout the plant world, but it is especially recognisable in tropical plants. The analyst notes differences in the amount of active principle in samples of drugs, the tanner in barks, while the wood-cutter accounts for one log being more durable than another by false notions of the moon's influence.

Leaving the trees of the forest, which are so difficult to investigate particularly, and coming to the epiphytes which grow upon them, we find specialisation carried to extremes in the great orchid family. The collector, the dealer, and the grower will all admit individuality here. The varieties are so numerous as to be almost impossible to define. Not only are there differences between plants of the same species from several localities, but even among those collected within a small area. Any grower who knows his plants will recognise one from another, and be continually on the look-out when each flowers for the first time. On account of these differences, orchid collecting is particularly fascinating. A dozen plants of one species are bought at auction, and, until they flower, no one can tell how they will turn out. We often hear of unique specimens, but the fact of the matter is every one is unique.

In the case of the orchids, there can be no suspicion of man's interference. Artificial selection may have been concerned in the development of other families, but not of this. The wonderful variations in the leaves of crotons and flowers of the hibiscus family may, perhaps, be due, to some extent, to their appreciation by the Polynesians, but the Indians of Guiana wear no garlands, nor do they cultivate a single flower for ornament. It is not only among the more showy genera, such as *Cattleya* and *Odontoglossum*, that individuality is conspicuous, but brassias, gongoras, and even some of the more insignificant families, are equally variable.

It is only in our gardens that we can observe the many variations of large trees, and here we have, unfortunately, but few species from the forest. By studying the vegetation on every trip to the bush, we

see that, although there are differences between wild specimens and those under cultivation, these are generally unimportant. As we stated above, they have their two seasons every year, when they drop their leaves, flower, and later ripen their fruits. Flocks of parrots and other frugivorous birds indicate by their congregating that the fruit season has arrived, while a great increase of fish in the creeks confirms the fact. Haws and other berries of temperate climates often remain on the bushes for months after they are ripe, but most tropical fruits fall quickly. Birds and monkeys would suffer greatly if the season lasted for only a month or two. There appears to be little migration in search of food, although this takes place to some extent. Individuality among the trees is, therefore, of advantage to animals, as it lengthens the season and prevents anything like a dearth for a long period. Birds and monkeys hardly ever alight on the ground, so that once the seed or fruit has fallen it is lost to them and eaten by rodents, or carried away to feed the fish which congregate in every little stream.

Young trees are at first very erratic, opening their flowers and shedding their leaves regardless of the proper season. Some, like the flamboyant (*Poinciana regia*), are never bare, although the older trees lose their leaves entirely and remain so for several weeks. As they become mature their habits are more and more regular until individuality is lessened and they become part of the crowd. In studying the flowering and fruiting seasons, therefore, the naturalist has to generalise from trees of full age, and avoid all deductions from erratic youth. It is almost as difficult to find out their characters as those of young animals at a similar stage.

Among the difficulties of the naturalist are changes produced by removal. He cannot study the epiphytal orchids in the forest, but must bring them to his garden, and thus change their environment. Heredity and individuality will, of course, be the same under all circumstances, but the other great factor in evolution must be interfered with. Few orchids are fertilised when growing on the coast, probably because the insects of the forest are wanting, but these go to prove what a wide field for observation there is among this family alone. Almost hidden among the foliage of a tropical garden, however, some orchids will attract a crowd of insects as soon as their flowers are open. Some of these can hardly be found at other times, but let the flowers open in the early morning, and they call attention to the fact by humming and buzzing around them.

The student of nature in the tropics should be something more than a specialist. Flowers must be studied in connection with insects, and fruits together with birds, bats, and even fishes. The interdependence of animals and plants is so close, that they seem to be engaged in a race where each tries to outstrip the other for his own advantage. The tree invites the perfect insect to fertilise its flowers, but warns off the larva by nauseous secretions in its leaves. Some

caterpillar or other is, however, almost always found to digest the most repellent juices and make all such efforts fruitless. A few have attained a large measure of success on account of the toughness of their leaves and their covering of stiff bristles or thorns ; but the most curious provision is, perhaps, that by which a garrison of carnivorous ants is accommodated. Either in swollen petioles or bases of thorns the ants take up their abode and fall upon and destroy every invader.

It is generally found that the trees most commonly attacked by larvæ are those the flowers of which are nectariferous, and therefore provide food for the perfect insect. The fiddle-wood tree (*Citharexylon*) is a striking example of this. Its spikes of white flowers perfume the air at night for long distances, and attract large numbers of moths. These same insects lay their eggs upon the tree, with the result that myriads of larvæ are produced, and the tree is often denuded of every leaf. In the ordinary course of nature it drops its leaves and is bare for a few days, after which the flowers are produced. The result of the ravages of the larvæ, like that of a ruthless pruning, is similar, so that by the time the perfect moth emerges from its pupa-case the flowers are ready to provide nectar for it. Other trees are attacked in the same way, and careful observation will no doubt show that there is something so remarkable in the actions of both plant and insect that nothing short of design can account for it.

Plants are undoubtedly improved by having to contend with so many difficulties. The survivor is always the fittest and most suited to the environment. One contrivance after another is developed, with good, bad, or indifferent results, but generally somewhat to its advantage in over-reaching its neighbours. Meanwhile, however, its rivals have not been sleeping, but as fast as the one gains an advantage others will have put on some defensive armour, or perhaps become more fitted to assume the aggressive. The study of the innumerable contrivances by which the plants of Guiana have attained their present stage is a never-ending source of interest. Probably there is more to be learnt here in a few miles of forest, river, and swamp, than in the whole of Europe, while the difficulties of a thorough investigation of even one square mile are enormous.

In Europe a large number of trees are anemophilous, but here all are fertilised by insect agency. This is one of the reasons why insects are so plentiful. At some seasons great moths fly into our open rooms at night, often to fall a prey to the gas lights. Sometimes a table below the chandelier becomes littered with hundreds of small moths and other insects in an hour or two. If a tree is flowering outside, and happens to be one of those that are fertilised at night, the increase is most striking. The moths fly towards the white flowers and are apparently attracted farther by the intenser light of lamps. In the streets of Georgetown the arc electric lamps also attract great numbers of insects, including some of the largest moths, which may be seen wheeling round them almost like

swarms of bees. By far the greater number of insects are nocturnal in their habits, and, as might be expected, white flowers are very plentiful. This makes observation so very difficult, as few of the insects can be seen at work. Now and again, however, a belated moth may be seen taking a last sip in the early morning, but such cases are rare.

After the contrivances for fertilisation, those by which fruits are protected and disseminated are most interesting. Nearly all those of our tables have thick skins, often charged with acrid secretions. These have acted so well that fruit-eating flies and wasps are almost wanting in Guiana. Birds, however, peel some of them, while bats succeed in utilising all except perhaps those of the orange family. In the forest the variety of pods, capsules, berries, and other seed-vessels is almost overwhelming. Perhaps the most useful developments which are common to almost all are those by means of which they float and are carried long distances by water. This object is generally attained by means of spongy pericarps, but many other devices are common, all helping to spread the seeds over very wide areas. In the Lecythideæ we have apparently protective contrivances against monkeys. The Brazil nut (*Bertholettia*) has a hard, woody capsule without an opening, which falls to the ground entire. In the genus *Lecythis* there is a cover which falls off when the nuts are ripe, and allows them to scatter in every direction. It is probable that monkeys try to get at these, and by loosening the covers let them fall out without being able to secure more than one or two. The cannon-ball tree (*Couroupita*) has a softer covering, but this smells so very disagreeably that neither monkeys nor rodents will touch it. Other natural orders repel animals in different ways. The short brittle hairs on the cowhage (*Mucuna*) pods would undoubtedly prove more than distasteful to bird or beast, as would also the prickles on the seed-vessel of the *Allamanda*. Sometimes the seed itself is protected, while the covering or pulp surrounding it is obviously intended to attract. In such cases birds assist the tree by scattering its seeds, and are therefore invited. In the bignonias, so common along the banks of the rivers, the seed has a delicate membranous expansion, on which it floats in the air like a butterfly. Other climbers have feathery appendages, while the stately *Triplaris* lets its seeds sail down like shuttlecocks.

Fibrous coverings to the stems are common among the tropical weeds. Exposed as these are to vicissitudes unknown in the forest, they have developed contrivances distinct from those of the trees. The most common mode of overcoming the difficulty of a dry season when the roadsides can no longer supply sufficient moisture is the faculty of ripening seeds before this takes place. In the forest, every plant is a perennial, but annuals are common on the dry savannahs, as well as on waste land in the towns and villages. Many weeds are not only repellent to man, but even to domestic animals

and other plants. They are so aggressive in their very nature that they overpower the more delicate plants. All along the coast of British Guiana, where a narrow fringe is under cultivation, hardly a native plant is to be seen, but a host of tropical weeds covers the parapets of the roads and every piece of ground otherwise unoccupied. They even penetrate long distances into the interior where there is a clearing, ousting the more delicate ferns and grasses natural to the locality. These latter may occupy the provision ground of the Indian without interference, but where the white man or negro makes his home the weeds follow him. One of the most common in pastures is the black sage (*Varronia curassavica*), which stands up defiantly, as if it knew that not even a goat or donkey will touch it. But of all these pests the sour grass (*Paspalum conjugatum*) is, perhaps, the worst. It is so rampant in the wet seasons that it allows nothing else to exist where the ground is moist. Disliked by every animal, it covers all the road-sides, and is a perpetual eye-sore to the agriculturist. Other weeds affect dry places, holding their own by pushing thick roots or tubers far below the parched surface. Like the forest trees, weeds struggle with each other, and, according to the season, either come to the front or give place. Where there is not much traffic on the roads, the grasses and other weeds cover them entirely, and even in the city they encroach so far that periodical cleaning is necessary. The dams of plantations are often almost impassable on account of tall grasses, which rise to a height of five or six feet. Many of these have serrated leaves, and the pedestrian, in pushing his way, gets his hands and face disfigured with scratches which in some cases may prove serious cuts.

As in the forest the trees take care not to allow the sunlight to penetrate and raise up a host of rivals, so on the plantation the agriculturist must keep the soil occupied to prevent weeds from spreading. The labour of weeding, when this is not done, is most arduous. In Europe, most of the pests of arable land are annuals which can be easily destroyed by the hoe. Here, however, the tufted grasses and sedges, deep-rooting shrubs, and tangled creepers are exceedingly difficult to eradicate. Only the dense jungle of a cane-field, or the shade of thick-growing trees, can keep them under without man's interference. Not a single weed shows itself among the sugar-canes after they have got beyond their earlier stage, and when ripe no large beast, much less man, can penetrate them. Here, instead of individuality, we have something like combination for an object, examples of which are more common in temperate climates than in the tropics.

James Rodway.

V.

The Distribution and Generic Evolution of Some Recent Brachiopoda.

WE had recently occasion to summarise the salient features of some modern publications on the anatomy and development of the Brachiopoda.[1] The past year was certainly remarkable for the production of excellent memoirs on this group of organisms. The great work, modestly entitled by Professor James Hall "An introduction to the study of the Genera of Palæozoic Brachiopoda," noticed in the October number of NATURAL SCIENCE (p. 628) is truly of an epoch-marking character, and a distinct revelation that the problem of the origin of species, so far as the Brachiopoda are concerned, is becoming merged in that of the evolution of genera.

It may be as well to recall the fact that the results of three American dredging expeditions and several French marine explorations have been worked out since Davidson's Report on the "Challenger" Brachiopoda was issued in 1880. It is from a review of the material obtained by the "Travailleur," the "Talisman" (2, 3), the scientific mission to Cape Horn of the "Romanche" (4), and three of Prince Albert 1st of Monaco's "Hirondelle" voyages (5) that the well-known French conchologists, Dr. Paul Fischer and D.-P. Œhlert have jointly derived the conclusions to which we are about to refer. It is fortunate that the rich harvests of results gathered in such distant regions should have been all entrusted to the experienced hands of the authors of the "Manuel de Conchyliologie et de Palæontologie conchyliologique." They have thus been enabled to formulate generalisations of value concerning the transition of genera, the distribution of some recent species, and their relations to the fossil forms of the Tertiary epoch. It is, perhaps, advisable to premise that Bayle's name *Magellania* is employed throughout to replace that of the far more familiar generic appellation *Waldheimia* of King, the use of which has been abandoned by Messrs. Œhlert, Fischer, Dall and others, because it had been previously given a few years before to a genus of hymenoptera; although, as M. Eugène Deslongschamps has observed, it would seem impossible that anyone could confuse a

[1] NATURAL SCIENCE, vol. i., p. 603.

Brachiopod with that most ill-met insect. We adopt the new name for the sake of uniformity, and in order to avoid the undesirable repetition of the bracketed tri-nomen which is making too frequent appearances in modern scientific literature. The substitute possesses the unusual merits of commemorating the exploits of a celebrated navigator, and of indicating at the same time the region of the maximum development of the genus it distinguishes; for the Magellanian province, as Darwin first pointed out, is remarkable for the great size of the mollusca inhabiting it, and to this rule the Brachiopoda form no exception.

First in interest and importance is the report of Messrs. Fischer and Œhlert on the Brachiopoda of the North Atlantic Ocean, which forms the third fasciculus of the fine series of Monographs embodying the results of the scientific explorations of Prince Albert 1st of Monaco on board his famous yacht "L'Hirondelle" (5). It gives a critical revision of the recent *Terebratulinæ*, accompanied by exquisite illustrations, and reveals some surprising facts in the general distribution of North Atlantic Brachiopoda, more especially in the Gulf of Gascogny, the archipelago of the Azores, and off the shores of the Soudan (5). The Gulf of Gascogny appears to be one of the richest localities, no less than sixteen species occurring there, including abyssal forms.[2] By far the most remarkable among them is the beautiful species founded on a single example first dredged by the "Challenger" Expedition from 390 fathoms off Culebra, one of the West Indian islands, and originally described by Davidson as the largest *Terebratulina* known (1).

This is undoubtedly a very interesting type. Fischer and Œhlert had several examples for examination, and were able to study the animal in detail, which was described by Willemöes-Suhm, in his MSS. notes, as relatively very small. It is now considered to be the type of a new genus, and of a new family (3), "characterised by its non-annulated loop and the arrangement of the arms, which, instead of forming free lobes, are re-united, and, rolled up in the pallial cavity, present a short sub-rectangular disc, a unique feature among the recent Brachiopoda. This disc can be schematically considered as composed of two rudimentary arms, joined laterally, and furnished with a large brachial sinus" (5). The figures now given show great incurvation of the beak area, and thickened margins of the valves—features which are also apparent in Davidson's drawings of the species. Hence we are inclined to consider *Dyscolia wyvillii* to be one of those atavistic types which Dr. Beecher states occur generally in families after the maximum of genera and species is attained.

This is a widely-distributed species, ranging from the West

[2] Twice that number of species are recorded, from Japanese and Corean waters, by Davidson in his "Monograph of the Recent Brachiopoda," *Trans. Linn. Soc. Zool.*, vol. iv. (1886–1888), p. 33.

Indies to the Gulf of Gascogny. It occurs off the East African coast, on the shores of the Soudan, and also in the latitudes of the Azores, another prolific region, yielding twelve species of Brachiopoda, of which four are also common to the West Indies, and eleven to European seas. A species dredged off the Portuguese coast, and named *Terebratula sub-quadrata* by Gwyn Jeffreys, is now regarded by Fischer and Œhlert as very closely allied to *Dyscolia wyvillii*, and the same authorities recognise *Terebratula (D.) guiscardiana* from the Sicilian Pliocene as an ancestral form of this interesting family Dyscoliidæ. The authors concur with Davidson in regarding the *T. septentrionalis couthouy*, which is larger and more rounded than the elongated and sub-pentagonal *T. caput-serpentis*, as a distinct species. They note, however, the occurrence of the varietal form they have named *T. germana*, from the Cape de Verde islands, and suspect it to be allied to the examples of so-called *T. septentrionalis*, recorded from the South Atlantic (Cape of Good Hope), the Indian Ocean, and Prince Edward's Island (5).

Sixteen deep-water species were obtained during the "Talisman" and "Travailleur" expeditions, three of which, *Rhynchonella cornea*, *Eucalathis tuberata*, and *E. ergastica*, were previously little known (2). Thirteen of them, according to Messrs. Fischer and Œhlert, occur in a fossil condition in the Pliocene marine deposits of Sicily and Calabria. These species are either absolutely identical, or at least exhibit such slender differences as permit their describers, in calling them by different names, to indicate a common origin. The Zanclean Sands of South Italy yield an assemblage of fossil forms which recalls the abyssal fauna of the marine Lusitanian province. We learn (2) that three species, *R. sicula*, *Dyscolia guiscardiana*, and *Muhlfeldtia (Megerlia) granulata*, have become extinct in the Mediterranean, while such closely allied forms as *R. cornea*, *D. wyvillii*, and *M. echinata* continue to be perpetuated in the Atlantic Ocean. Three other species found in the Pliocene of South Italy, *Magellania peloritana*, *M. cuthyra*, and that widespread and interesting species *Liothyris sphenoidea*, which was long known as a fossil before it was dredged in the living state, appear to be absolutely on the point of extinction in the Mediterranean (2), although they are still represented in the broad Atlantic under the designations of *Magellania septigera*, *M. cranium*, and *Terebratula cubensis*.

Hence it is evident, the French conchologists conclude, that the Mediterranean has lost some of its deep-water species since the Pliocene period. This tendency is still manifested, and it is held to be associated with the gradual rise in the temperature of the Mediterranean waters which is about + 13° C., from below 183 metres down to the bottom. The Mediterranean now resembles an inland sea as compared with the Atlantic, where the temperature decreases as depth increases. The fact of the general uniformity of deep-water temperature, first established by the "Challenger" explorations,

accounts for the wide distribution of the abyssal fauna. During the Pliocene period, the cold currents penetrated into the Mediterranean, which then received a number of hardy boreal species, the remains of which have been preserved in the fossiliferous deposits of Ficcarazzi, in Sicily. The elevation of the sea-bottom at the Straits of Gibraltar checked the inflow of the cooler currents of the ocean depths, with the consequent result of a gradual rise of temperature in the then nearly enclosed sea. The abyssal forms, being unable to accommodate themselves to the changed conditions, died out in the thermal waters, but their collaterals continued to flourish and multiply in the favourable and unchanged conditions of the Atlantic Ocean (2).

These considerations, Dr. Fischer and D.-P. Œhlert maintain, confirm the hypothesis that the distribution of marine species is principally regulated by the temperature of the waters. Hence it arises that the Mediterranean Sea now possesses a rich surface fauna and a poor abyssal one; while the Lusitanian province beyond is characterised by a poor surface fauna and a deep-water fauna of extraordinary richness and vitality.

The results derived from a careful study of the specimens obtained by the "Romanche" during the French scientific mission to Cape Horn (4) were of a different character, and, while affording some distributional data of interest, which serve to emphasise the great dissimilarity existing between the boreal and the austral brachiopodal fauna, yielded more important evidence concerning the transitional stages of development of the larger southern species of *Terebratella* and *Magellania*. These successive stages are shown to be of a more complicated character and to differ somewhat from those undergone by the northern forms as first described by Herman Friele, who based thereon his simple division of the great family of Terebratulidæ into two groups of the short loops and the long loops since generally adopted. It is now quite clear that the genus so long known as *Waldheimia* is a closed type, the ultimate phase of a long line of development through successive pre-magadiform, magadiform, magaselliform, and terebratelliform stages culminating in the southern seas in *Magellania venosa*, *M. lenticularis*, and *M. grayii*. It is remarkable that the species of the Northern Ocean should reach the same goal by a shorter line and somewhat different stages, which Œhlert and Fischer define as centronelliform, ismeniform and terebratelliform, ultimately attaining the Magellanian grade as represented by *Wald. septigera* and *Macandrewia cranum* (4).

Davidson's classification of the long-looped species of *Waldheimia* with the short-looped species of *Terebratula*, *Liothyris* and *Terebratulina*, which assume their stable generic loop characters without undergoing any metamorphoses, cannot possibly be longer sustained. It is quite evident that *Magellania* has absolutely no affinities with the sub-family Terebratulinæ, but must be associated with the Terebratellinæ and should rank above that genus in a natural scheme of classification, as

Dall suggested in 1891.[3] The following subdivisions of the great family Terebratulidæ, which we should place in the following order, were proposed by Œhlert in 1887:—Terebratulinæ, Centronellinæ, Magasinæ, Muhlfeldtinæ, Terebratellinæ and Magellanæ. The present investigations confirm this grouping. The genus *Terebratula* of the first-named and *Centronella* of the second are the more ancient terebratuloid types, and were represented in the Silurian seas.

Messrs. Œhlert and Fischer received the materials for their research on the Brachiopoda of the Magellanian province too late to permit of the publication of their conclusions in the series of memoirs relating to the French scientific mission to Cape Horn, during which the "Romanche" dredged often in many localities. Few species were obtained, but the number of individual specimens of different ages was large, and afforded them much desired opportunities for studying the metamorphoses of the loop and gradational development of *Terebratella dorsata* and *Magellania venosa*. The knowledge thus gained, it is distinctly stated, should tend to the suppression of many so-termed species and some assumed generic forms, for "the more the Brachiopoda are studied the more the number of legitimate species will decrease." It is demonstrated that the same individual in the course of its post-embryonic development assumes successive stages which are identical with both specific and generic forms of various authors. Thus it is fully proven that the genera *Waltonia* (Davidson) and *Magasella* (Dall) are really immature *Terebratellæ* in which the brachial apparatus has not completed its development. Gould's *Terebratella pulvinata* is shown to be merely a stage of *Magellania venosa*, which is not permanently arrested in the terebratelliform grade like its congener *Terebratella dorsata*, but assumes ultimately the characteristics of *Magellania venosa*. This is a fine but variable species with a large number of synonyms which have been well worked out by Davidson. Messrs. Œhlert and Fischer state that it attains maturity early, and its complete evolution was observed by them in specimens 27 mm. long in which all traces of the jugal band had disappeared. The largest known specimen measures 3 inches 2 lines in length and 2 inches 8 lines in breadth. *Magasella gouldi* becomes *Terebratella gouldi*, but it is not yet known whether it stops there or eventually assumes the generic characters of some species of *Magellania*. It is certain, however, that *Magasella evansi* is but a phase of *Terebratella cruenta*. *Magasella patagonica*, *M. flexuosa*, and *M. suffusa* name different ages of *Terebratella dorsata*, *M. lævis* another stage of *Magellania venosa*, while *M. adamsi* first becomes terebratelliform and eventually passes into *Magellania grayii* (4).

In the course of these investigations the important physiological facts of accelerated growth were recognised. Owing to favourable conditions of the environment, some adolescent individuals were

[3] The name *Terebratulinæ* was accidentally printed for *Terebratellinæ* on line 19, p. 603, of my paper "Recent Researches on the Brachiopoda," in NATURAL SCIENCE, October, 1892. The error escaped notice.

found to be obviously capable of reproduction during the Magasiform and the Magaselliform phases, which precede that of *Terebratella*, and thus an inferior race of *Magasellæ* is perpetuated which never attains the characteristics of the adult *Terebratellæ*. The authors deem it advisable, therefore, to retain Dall's subgeneric name of *Magasella* for such permanently arrested types, although it is quite clear that four-fifths of the so-called species will prove, as the American authority admits, to be the immature forms of different species of *Terebratellæ*, with which genus, it is well to remember, they invariably occur associated. Some *Terebratellæ* also remain so throughout life, but with others the terebratelliform stage is transitory, and they pass up into true Magellanæ. Many such grades were arrested and became stable during geological periods, and should therefore retain generic value. Among these the authors cite *Centronella*, *Magas*, *Terebratella*, and possibly *Ismenia* of King.

This is a distinct advance in our knowledge of the Brachiopoda, and throws some light on the origin of genera. It illustrates at the same time the difficulties under which palæontologists labour in describing and naming obscure fossil forms, deprived of the assistance to be derived from a study of the developmental history of any member of the group in the living condition. The simultaneous occurrence, so often noticed, of a smooth series and of ribbed examples of the same species, indicate the futility of relying on these mutable external features as indicators of species. The most divergent views are taken of generic values; for instance, Mr. H. Douvillé proposed his genus *Neothyris* for *Waldheimia lenticularis*, a well-known form occurring in the New Zealand province, both in the living and fossil condition, allied to the *W. kerguelenensis*, and so closely related to the giant *W. venosa* of the Magellanian province, that Dall even considers it as a varietal form or Novo-Zealandian race of the S. American species. "The difference in colour seems the main distinction" (4). Œhlert's sectional employment of the term Neothyrine as differentiating the smooth *Magellanæ* like *venosa*, and *lenticularis*, from the pleated forms, such as *M. flavescens*, is far less objectionable.

Now that *Terebratulina wyvillii* has been definitely promoted, and with good reason, to be the type of a new family, the Dyscoliidæ, in which the short united "arms" resemble a freemason's apron, the fine *T. crossii* from Japanese waters is the largest known species of that sub-genus. It is remarkable that it should have been also dredged by the "Romanche" off the coast of Patagonia. *Liothyris moseleyi*, first obtained by the "Challenger" off Kerguelen islands, and subsequently by Dall in the West Indies (Blake Expedition), also occurs off Tierra del Fuego, and proves to be a well-defined, far-ranging species of the widely-distributed Liothyrine group of *Terebratula*.

The Magellanian province, to which the Marion isles, the Crozets and Prince Edward's Island are now attached by Œhlert and

Fischer, is poor in species but rich in individuals. Favoured by abundant marine vegetation, the species so numerously represented attain relatively gigantic dimensions. Seven species occur, one of which, *Magellania fontaineana*, Davidson held to be only a synonym of *M. venosa*. The French conchologists consider it distinct, and as still inhabiting the littoral. It is found fossil in Chili, and *Terebratella cruenta* occurs in far distant New Zealand. A close similarity, it is evident, exists between the brachiopodal fauna of the Magellanian province, where the species are practically colourless, and the bright-hued forms of the Novo-Zealandian. The rich colouring is regarded as an adaptation to the brilliant marine vegetation. The Kerguelen islands occupy an intermediate position, both as regards geographical situation and specific distribution, although the temperature of the bottom currents is identical with that of Tierra del Fuego. The fauna of the Magellanian province differs radically, alike from the Boreal and from that of the Tasmanian region, which is linked by its characteristic Kraussinoid types with the Cape of Good Hope, and connected with the Kerguelen islands by the occurrence of the same genus, a species of which abounds in the latitudes of St. Paul's islands. Fishes seem to be ardent collectors of Brachiopoda. Two specimens of *Magellania venosa* were found in the stomach of a cod in New Year's Sound. *Terebratulina septentrionalis* occurs frequently in the same situation off the banks of Newfoundland, and a much rarer species of the genus was originally described from a single specimen taken under similar conditions from a fish captured at a depth of 80 fathoms off the Mauritius.

A detailed comparison of the Austral species leads Messrs. Œhlert and Fischer to confirm the hypothesis "of the existence of an extended but hitherto little-known Arctic and circumpolar fauna." In a short paper, just issued, the authors summarise the results of their observations "On the evolution of the brachial apparatus of certain Brachiopoda" (6), and present a tabular view of the affiliation of the *Magellanæ*. They suggest the subdivision of the genus into two sections, differentiated by distinct lines of development and by the characters of the adult species. By their original and well-executed researches they have added considerably to our knowledge of the distribution and generic evolution of the recent Brachiopoda.

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AGNES CRANE.

V.

Aggressive Mimicry Among the Flies of the Genus *Volucella*.

MUCH attention has recently been drawn to the resemblances which exist between the forms of *Volucella* and certain species of Hymenoptera. Not only are theoretical explanations of the resemblances in dispute, but there is disagreement as to the exact species of *Bombus* which are mimicked. It may therefore be opportune to describe briefly the characters both of the Diptera and Hymenoptera concerned.

In England, there occur four species of the genus *Volucella;* of these, *V. inanis*, *V. pellucens*, and *V. inflata* possess a smooth or nearly smooth body, while *V. bombylans* alone possesses a markedly hairy body.

V. inanis is parasitic in the larval condition on the larvæ of wasps and hornets; the adult in no small degree resembles a wasp. *V. pellucens* and *V. inflata* are similarly parasitic on wasps, but neither can be considered to bear a resemblance to its hosts in the adult stages. The larva of *V. bombylans* is, in the same way, parasitic on certain species of the genus *Bombus*. The adult of this last occurs in two varieties; the dimorphism affects both sexes, and intermediate forms are occasionally found.

The one variety is black, with the apical portion of the abdomen red. It thus closely resembles, both in size and colour, a small worker of *Bombus lapidarius*, or of *B. derhamellus*, or of *B. sylvarum* (in some of its variations). The other, var. *mystacea*, has a yellow border to the otherwise black thorax, yellow hairs on the basal portion of the abdomen, which is itself in this region yellow and semi-transparent, and a grey or greyish-yellow apex to the abdomen. The exact extent of the yellow colouration is liable to variation both on the thorax and abdomen. Here the resemblance is to a worker of such species of *Bombus* as *B. pratorum*, *B. hortorum*, *B. schrimshiranus*, *B. terrestris* (*lucorum*), or *B. sylvarum* (in other variations). The two varieties present also a superficial resemblance, when on the wing, to two other bees, viz., *Anthophora retusa* and *A. pilipes*. The males of these two bees are clothed with brownish hairs, which fade to a yellowish grey in weather-beaten specimens, with a considerable mixture of black in certain lights; the females are entirely black

But it is rather in size, and especially in mode of flight, that *V. bombylans* resembles the *Anthophoræ*. In both cases the insects have a dashing impetuous habit, interrupted by periods of hovering. During such rapid movements exact markings fail to catch the eye, and the general impression produced by the two is by no means dissimilar.

It is further maintained by some observers that var. *mystacea* resembles *B. muscorum*. This bee has tawny yellow hairs on the thorax, while on the abdomen the hairs are yellow, more or less mixed with black, or even entirely black. Whether mimicry can be said to exist in this case is a matter of opinion, but it should be borne in mind that both *B. muscorum* and the *mystacea* variety of *V. bombylans* vary in appearance and colour-effect, according to the direction from which they are observed. A freshly-emerged *mystacea*, if viewed from in front, and from a low elevation, would probably be considered by very many persons to resemble *B. muscorum*, while, if viewed either from directly above or from behind, the resemblance would probably be denied. Moreover, a faded specimen of *muscorum* loses much of the tawny colouring and appears of a yellowish grey.

Both varieties of *V. bombylans* have been observed to emerge from nests of the three species of *Bombus* which are chiefly attacked, viz., *B. muscorum*, *B. lapidarius*, and *B. hortorum*. The first-named of these is apparently the most infested. In this connection the habits and demeanour of the bees are of interest. *B. muscorum* constructs a nest on or near the surface of the ground, and is of a comparatively mild and pacific disposition. The other two construct their nests at some depth from the surface, and are, especially *lapidarius*, fiercely irritable. Thus the nest of *muscorum* is clearly more accessible to *Volucella* on account both of the weakness of its strategic position and the faint heart of the defenders.

Be this as it may, it is exceedingly difficult to comprehend how the colour of *Volucella* can in any way impose upon any of the bees, for so soon as the fly has entered the approach to the nest it must be in more or less total darkness, and from that moment the value of mere colour and markings is reduced to *nil*. Any *Bombus* which encountered a *Volucella* in the darkness could not by any possibility discriminate by the sense of sight between friend or foe. It seems far more probable that the hairiness of *V. bombylans* (in both varieties) is the feature which should be spoken of as an example of "aggressive mimicry." It is the sense of touch which alone can enable the *Bombus* to detect *V. bombylans* in the semi-darkness, and surely the hairy body is less likely to arouse suspicion when touched than would a smooth body, for the latter could in no case enter the nest of *Bombus* as belonging to one of the natural members of the colony.

This contention derives much support from the fact that the other three species of *Volucella* mentioned above have hairless bodies and are parasitic upon hairless Hymenoptera. Our knowledge at

present is insufficient for us to be able to determine whether the hairiness of *V. bombylans* is as the "skins of the kids of the goats," or if the Esaus of the genus have practised the converse deception.

If this theory be correct, the colouration of *Volucella* must be removed from the category of "aggressive mimicry"; unless, indeed, the general bee-like appearance disarms suspicion when the *Volucellæ* are flying in the neighbourhood of the nests of the *Bombi*. It seems more probable that the dimorphic mimicry of *V. bombylans* is of "warning" value, and this is far more in accordance with the mimicry occurring in dimorphic forms among other groups of insects. During the past summer I made a few experiments to test this point. Specimens of *V. inanis* and of both varieties of *V. bombylans* were offered to six lizards whose ordinary diet consisted of worms, blue-bottle flies, and caterpillars. In each instance the *Volucellæ* were inspected from a distance and watched attentively, but not one was tasted or even licked. When the *Volucellæ* flew round the cages the lizards exhibited the utmost alarm.

These lizards were brought from Italy in the spring, and had not previously, while in my keeping, been alarmed by any bees or other insects capable of inflicting injury upon them. It was interesting to observe that the wasp-like *V. inanis* was evidently regarded with greater fear than *V. bombylans*. The same dread of any insect banded with black and yellow, *e.g.*, larva of *Euchelia jacobeæ*, was frequently exhibited.

It is evident that as yet we are not in a position to dogmatise concerning the mimicry exhibited by these (and many other) insects. Now that attention has been called to our previous somewhat hasty conclusions, it is to be hoped that the work will be taken in hand by many independent observers during next season. The *Volucellæ* abound in nearly all country places, and are easy to keep under observation in captivity. An interesting point to record, among others, would be the relative proportions of the sexes in the two varieties of *V. bombylans* and the *mystacea* variety. In a series of fourteen taken last summer, consisting of eight *V. bombylans* and six var. *mystacea*, all the former are males, but only one of the latter. Theories to explain mimicry we have in abundance; let us hope for more facts in the future.

OSWALD H. LATTER.

VI.

The Rothschild Museum, Tring.

SO considerable a part of the richness of our public collections in Natural History is due to the interest and munificence of private individuals, that every true student will welcome with gratitude each new recruit in the ranks of those who devote their energies and resources to the amassing of materials for investigation. Of these disinterested workers, who not merely collect but also pursue scientific research, there are happily many still rising in Britain to replace the older generation now quickly passing away: and conspicuous among those in the forefront of progress must be placed the Hon. L. Walter Rothschild, whose interest in the cause of Natural History has of late attracted so much attention. Since his undergraduate days at Cambridge, when he came under the influence of Professor Alfred Newton, Mr. Rothschild has devoted himself with enthusiasm to the pursuit of Ornithology and Entomology; and while following these studies he has not remained satisfied with the resources of the University and National Museums, but has amassed great collections which in some respects rival, and even surpass, those of the British Museum itself. His endeavours have met with so much success, that he has recently erected a commodious museum at the town of Tring, his place of residence, to contain the collections; and he has now added to his private cabinets of Ornithology and Entomology a fine series of typical animals of other groups, arranged for the instruction of the general public, who are admitted on four days of the week. Few naturalists have hitherto visited the museum, the study-cabinets being still incomplete; but, by the courtesy of Mr. Rothschild, we have lately enjoyed the privilege of seeing the arrangements so far as advanced, and it will be of interest to those engaged in research to learn the present position and prospects of the institution.

The Museum and "Cottage," of which we give a side-view in Fig. 1, is a red brick structure, about 100 ft. long by 50 ft. broad. Attached to one of the outer walls is a taxidermist's and general workshop; while, a short distance away, a space of ground, occupied by cages and a paddock, is arranged for the accommodation of living animals, kept for the observation of their habits and life-history. The museum itself consists of one large room, 65 ft. by 32 ft., with a gallery all round, some 15 ft. from the floor. Two large glass cases, about 50 ft. long, occupy the central floor, and the whole of the wall-space, both on the ground floor and in the gallery, is one continuous

glass case of moderate depth. There is, therefore, scarcely a square inch of waste room anywhere. The cases themselves are beautifully fitted with mahogany, somewhat similar to those at the Natural History Branch of the British Museum, though considerably improved. The tops of the central cases are solidly constructed, and by this neat device form, as it were, a central floor to the gallery above, occupied by large specimens too bulky for the glass cases themselves. Additional specimens, chiefly large fishes, are suspended from the roof at a level with the eye of the visitor in the gallery, as shown in Fig. 2.

Entering the museum on the ground floor, one finds the whole of the right-hand portion devoted to birds, and the whole of the left-

FIG. 1.—THE ROTHSCHILD MUSEUM, TRING.

hand half to mammals; while upstairs in the gallery the typical series of sponges, corals, molluscs, fishes, and reptiles are in the wall case, and the insects, with crustaceans, occupy small boxes with loose wooden doors (to exclude light), fixed to the hand-rail of the gallery balustrade, as shown in the foreground of our illustration. On the side galleries these boxes are hinged to permit of their being lowered to hang parallel with the balustrade; such an arrangement not only allowing an uninterrupted view of the specimens placed on the top of the central cases, but permitting the top-light to fall freely on the ground floor.

The whole of the exhibits in the Museum cases, with some exceptions among the mammals and birds, form what Mr. Rothschild

terms his "Public Collection"; and this is regularly open to the inspection of visitors. The private cabinets for study are arranged in the "cottage" at the entrance, and in the "bird rooms" at the opposite end of the museum. The Bird Room on the ground floor is already fitted with cabinets and provisionally arranged. The collection comprises about 30,000 skins of from 5,000 to 6,000 species, and is especially rich in "types." The specimens are preserved, the larger ones in glass-topped drawers, and the smaller ones in light glass-topped drawer-trays, the birds lying in rows in cardboard grooves. This arrangement is found peculiarly convenient, permitting, as it does, the whole of the individuals of a species to be

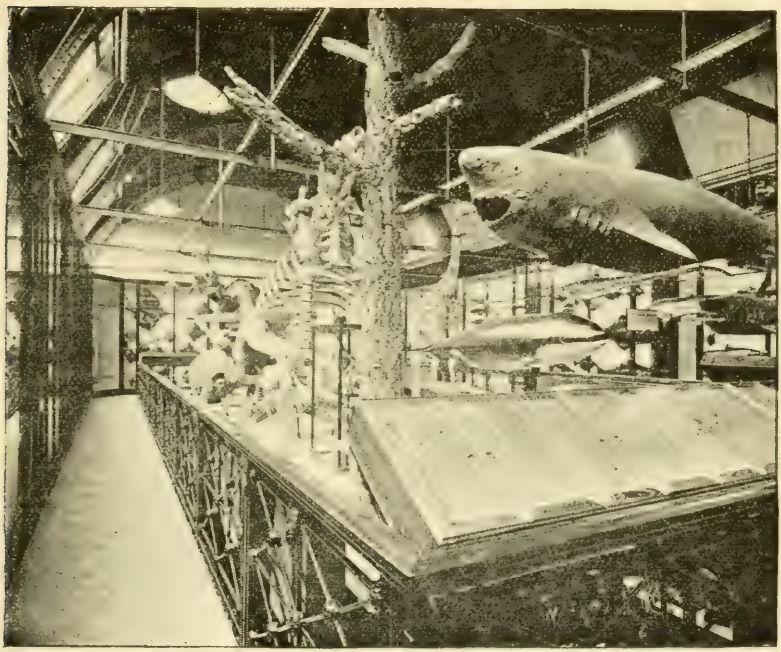

FIG. 2.—THE UPPER PUBLIC GALLERY OF THE ROTHSCHILD MUSEUM, TRING.

lifted out at once and removed in their own tray to the student's work-room and library. The upper bird room is as yet merely a store-room, and above this is another small store-room in the roof, reached by a spiral staircase.

The "cottage" is separated from the Museum by a wide stone staircase, which forms the public means of access to the gallery. It contains Mr. Rothschild's own study, the curators' room, the library, and the entomological cabinets. The library is certainly the most useful and extensive collection of Natural History serials we have seen in a private institution; the special works on Ornithology and Entomology are mostly represented, and are being constantly added

to. The Coleoptera are arranged in glass-topped boxes in bookshelves on the staircase and in the upper room, while the Lepidoptera are in course of arrangement in cabinets, both in the upper and lower rooms, the collection containing altogether about 200,000 Lepidoptera and 300,000 Coleoptera, and especially noteworthy for the large and complete series of most species. All the specimens placed in the public collection are, of course, duplicates.

Passing to a more detailed examination of the public museum, the first striking feature is the wonderfully life-like aspect of the stuffed birds and mammals. This is due to the fact that Mr. Rothschild employs a specialist in each group, and for each kind of work. For instance, all birds of the duck family, ostriches, &c., are stuffed by Doggett, of Cambridge; the eagles in Switzerland; and the same care and discretion are exercised in reference to the mammals. The fishes, also, in the gallery are, many of them, remarkably good, the work for the most part of the museum attendant. Chief among the mammals may be noticed the large (probably the largest known) gibbon, *Hylobates syndactylus*, brought by Dr. Hartert from Sumatra; while close by, on a small board, there sits in most life-like posture the late chimpanzee, "Sally," from the London Zoological Gardens. The lower monkeys are also numerously represented. Among the Carnivora, a particularly interesting specimen is a hybrid between the lion and tiger; and there is a bear's head from Central Siberia of unusual size, the skull being larger than that even of the largest Cave Bear in the Paris Museum, and said by Milne Edwards to be almost identical in characters with the latter. There is a fine example of the Caspian seal, the only specimen in England; and albinos are numerous, the most interesting being one of the sable. Among the unique specimens of Ungulata, we may refer to a fine quagga—an animal so rare that it is unrepresented even in the British Museum—and the head of a rhinoceros from Central Somali-land, with an enormous straight horn, and possibly of a new species. Among smaller mammals, the recently-described opossum-mouse from New Guinea (*Acrobates*) is conspicuous; and there is also an example of the rare squirrel, *Rhinosciurus tupaioides*, from Borneo. The collection of marsupials is very extensive, comprising a fine series of kangaroos, wallabies, and koalas, &c.; while the monotremes comprise the unique *Echidna nigroaculeata*, lately described by Mr. Rothschild, from New Guinea—a very large animal that lived for some time in Mr. Rothschild's possession, and of which he has carefully preserved the skeleton and the soft parts in spirit.

Among birds, the most remarkable series are those of hybrid pheasants, chiefly bred at Tring Park; of the Tetraonidæ (capercailzies, &c.) from Russia, comprising about 280 specimens; and of the ostriches and other struthious birds. The latter include a fine series of cassowaries, stuffed by Mr. F. Doggett, of Cambridge,

and coloured from life by Mr. Frohawke: the preservation and colouring of the carunculated skin of the neck form one of the greatest triumphs in the taxidermist's art. Young emus, showing the striped character of the early plumage, are also interesting; and to illustrate the development of the struthious birds in the past, Mr. Rothschild has added a model of the skeleton of the New Zealand moa, other moa bones, and a complete egg of the great extinct Madagascar bird, *Æpyornis*. In an adjoining case the display of eagles also deserves special notice.

Passing to the gallery, there are a few striking fossils occupying the walls of the staircase, notably *Ichthyosaurus* and *Steneosaurus* from the Lias, and a large palm-leaf from the Eocene of Monte Bolca, near Verona. A striking object here also is an enormous stuffed python, from Western Africa. The large specimens to be seen from the gallery scarcely require detailed enumeration, the general arrangement being shown in the accompanying Fig. 2. Mr. Rothschild, like a true naturalist, does not regard extinct animals as belonging to a totally distinct domain from those now living; and he thus places, for comparison with the sloths and armadillos, models of the skeletons of *Megatherium* and *Glyptodon*. There is also a noteworthy extinct giant tortoise, *Testudo grandidieri*, from Madagascar, represented only by the shell; the limbs of this animal (it is said) having, by accident, been offered to and purchased by the British Museum. Around the walls are the fishes and reptiles, only a small space being reserved for the other groups, which, as we mentioned above, are confined merely to typical examples. Of the fishes, the best things to be seen are a saw-fish, some sturgeons, some excellent specimens of European species, and a few in spirits. In the hanging boxes on the balustrade the insects are remarkable for their perfection, and here the same rule is carried out, that of showing only typical examples of the various orders.

All these collections are open to the public during certain hours on Mondays, Tuesdays, Wednesdays, and Fridays, and it is gratifying to find that Mr. Rothschild's liberality is appreciated. Since the formal opening early in September, no less than 16,000 visitors have been admitted, and the largest attendance is on the Wednesday evening from 4 to 7, when as many as 300 persons frequently pass the door. The Museum is lighted throughout by electricity, and the rows of small incandescent lamps are most conveniently and admirably arranged.

Referring next to the purely scientific collections, it must suffice merely to enumerate the various series of specimens they comprise. Among birds may be mentioned the following:—

(i.) The Bartlett Collection of weaver birds and finches.

(ii.) The Holdsworth Collection from Ceylon.

(iii.) The Bruijn and Beccari Collection both of birds and mammals from New Guinea, containing many type-specimens of birds described by Count Salvadori.

(iv.) The Whitehead Collection from Palawan and Borneo, with numerous types.

(v.) The Von Hügel Collection from New Zealand.

(vi.) Sir Walter Buller's Collection from New Zealand, containing the types described in the second edition of his classic work.

(vii.) The Palmer and Scott Wilson Collection from the Sandwich Islands and Chatham Island.

(viii.) An extensive collection made by Dr. Ernst Hartert in the Dutch West Indies.

Among insects may be mentioned :—

(i.) The Felder Collection of Lepidoptera and Coleoptera.

(ii.) The Boucard Collection of Coleoptera.

(iii.) Part of the Doherty Collection of Lepidoptera from the Malay Archipelago.

(iv.) The Rev. W. Hamilton's Collection (in part) of Lepidoptera from the Khasia Hills.

Mr. Rothschild, it may be added, is now employing special collectors in the Sandwich Islands and Madagascar, and his series of birds from the first-mentioned islands is already unique.

Enough has now been said to show how vast are the collections already acquired for the Tring Museum, and it only remains to observe how thoroughly Mr. Rothschild is carrying out the undertaking he has begun. Up to the present most of the material has been stored and comparatively inaccessible; but by the appointment of Dr. Ernst Hartert, the well-known ornithologist, as curator, the initial step towards order and arrangement has been taken. The cabinets of bird-skins have already been provisionally arranged; and early next April Dr. Jordan, of Hildesheim, will enter upon his duties as assistant-curator in charge of the entomological collections. The general attendant, caretaker, and taxidermist will also shortly be provided with two additional men. In short, there will be an adequate staff to preserve and render accessible every part of the collections.

It is the custom of certain official naturalists to speak disparagingly of those who, from personal taste, begin to form private collections, afterwards to find themselves deeply engrossed in the subject, and competent to speak with authority on special groups of animals. They declare that private collections are useless, being inaccessible and neglected. We have no sympathy with such expressions in any case; but more especially would we protest against the injustice of those who, even in public meetings, give utterance to these sentiments in reference to the Tring Museum. All the collections are preserved with the greatest care and arranged in the most approved and convenient fashion. As soon as Dr. Hartert and Dr. Jordan have had a brief time for work, everything will be as readily accessible as in the British Museum; and we are permitted to add that, when this arrangement is accomplished,

Mr. Rothschild's collection will not merely be for his own private use and that of his curators, but will be at the disposal of every competent specialist who desires to make use of it for reference. While arranging the collection, Mr. Rothschild and his curators will prepare a catalogue and describe the forms that appear to be new; and the systematic manner in which the collecting is being undertaken in little-explored regions abroad is certain eventually to yield most important results.

In conclusion, we would advise all our readers who are interested in mammals and birds, in the art of taxidermy, or in the arrangement of exhibition cases, to visit the Tring Museum. It is a remarkable and beautiful collection, of interest alike to the wandering amateur and the professed naturalist.

A. Smith Woodward.
C. Davies Sherborn.

SOME NEW BOOKS.

THE VISIBLE UNIVERSE: Chapters on the Origin and Construction of the Heavens. By J. Ellard Gore, F.R.A.S. Demy 8vo. Pp. 346, with 6 stellar photographs and 12 lithographic plates. London: Crosby Lockwood & Son, 1892. Price 16s.

MR. GORE is well known as an enthusiastic observer, an able computer, an authority on variable stars, and the author of successful works which appeal to the general reader and the amateur astronomer. The present volume, like its predecessors, is well written, well printed, and well illustrated. Its main object is to give an account of various hypotheses as to the origin and construction of the heavens, and of the arguments for and against them. In discussing various astronomical fictions, as these hypotheses may be termed, many facts are necessarily described and much interesting information is communicated. Thus, in reviewing the "Meteoritic Hypothesis," the author gives an account of the leading characteristics of stellar spectra and of the recent work by Professor Lockyer, Dr. Huggins, and others, on such subjects as the coincidence of the chief nebular line with the brightest termination of the magnesium fluting.

Mr. Gore has paid special attention to the distribution of stars in space, and this subject is accordingly treated at considerable length. The theories of Wright, Lambert, Herschel, Struve, Proctor, and others, as to the construction of the visible universe and the laws governing the distribution of stars, are described and critically examined. There is a very interesting chapter on stellar parallax and stellar motions in which the latest information on these important subjects is given. Speaking of the sun's motion in space, the author asks, What was the position of our system in past geologic time? If the motion has been rectilinear we must have come from that part of space in which Sirius is at present situated. "Now, with a parallax of 0·39" the distance of Sirius from the earth would be about 49 billions of miles. With a velocity of 14 miles a second the sun's annual motion would be nearly 442 millions of miles. Therefore in 200,000 years the distance traversed would be about 88 billions of miles, which, carried back, would place it out in space far beyond the distance of Sirius." It is thus safe to say that the Ichthyosauri of the Lias must have looked upon a system of stellar distribution very different from that which we now see.

The book appeals to many classes of readers. Those who like to revel in metaphysical subtleties will find portions adapted to their peculiar taste; those who prefer to trace the work of individuals in the present state of our knowledge will turn to the historical parts; while those who feel that such subjects as the above are apt to become tedious, will find plenty to occupy their attention in the descriptive portions of the volume. Too many tastes are catered for to make the book uniformly palatable to any one individual.

Among the illustrations we may select for special commendation those which represent some of the recent results of the application of photography. The magnificent photograph of the nebula in Andromeda, by Mr. Isaac Roberts, forms an appropriate frontispiece.

SHORT STALKS; or, Hunting Camps North, South, East, and West. By E. N. Buxton. 8vo. Pp. vii., 405. Illustrated. London: Stanford, 1892. Price 21s.

FROM the main title, which alone appears on the cover, it might be thought that the volume before us was a treatise on some branch of botany or horticulture, if not a novel, whereas, as a matter of fact, it is a very interesting book on "big game" shooting. It must not however, be thought that it comes merely under the designation of an ordinary book of sport, since the author gives us some very valuable information as to the habits and mode of life of the various animals treated. Moreover, Mr. Buxton has been fortunate in the localities he has selected for many of his hunting trips, whereby he has been brought into contact with animals as to whose habits there has hitherto been a dearth of information in the works of recent English writers. Sporting works on the larger mammals of Southern Africa, India, and North America are so numerous as to afford an almost superabundance of information; whereas, in regard to Eastern Asia, Europe, and North Africa there is a marked dearth of accessible and reliable observations by English sportsmen. We are, therefore, especially glad to welcome Mr. Buxton's accounts of the Sardinian Mouflon (*Ovis musimon*), of the Spanish or Pyrenean Wild Goat (*Capra pyrenaica*), of the Arui (*Ovis tragelaphus*) of North Africa, as well as of the Pasang or Persian Wild Goat (*Capra ægagrus*), and the Chamois (*Rupicapra tragus*). The majority of the twelve chapters into which the work is divided have already appeared as separate articles in various serials and newspapers, but they are now so much embellished by the addition of the excellent illustrations with which the book is adorned, that it is difficult to recognise them as the same. By the courtesy of Mr. Stanford we are enabled to reproduce one of these illustrations as a sample. In addition to these excellent portraits of the animals he describes so graphically, Mr. Buxton also incidentally introduces some charming little sketches of bird-life and scenery.

The first chapter of the book is devoted to the Mouflon, which the author describes as one of the most difficult animals to stalk which he has ever come across. The second deals with the Chamois of the Alps, while the variety inhabiting the Pyrenees is described in the ninth chapter under its Iberian name, Izzard; and here we may point out to the author that he is quite behind modern zoology in referring to this animal under the title of *Antilope rupicapra*. The third chapter is devoted to American game, where some interesting observations are recorded as to the habits of the Bighorn Sheep (*Ovis montana*); while in the fifth Mr. Buxton treats of the Arui and Mountain Gazelle of Algeria. The author appears to have been the first Englishman who has hunted these animals, and the results of his observations have already appeared in the Zoological Society's Proceedings. We are told that the name "Aoudad," so commonly applied to the Arui, is quite unknown in its native land, and the author also speaks as to the remarkable powers of concealment possessed by these animals. The Elk of Norway claims the whole of the fifth chapter; while the sixth (which originally appeared in the *Nineteenth Century*) treats of the Pasang, under the legend of "The Father of all the Goats," in allusion

MALE CHAMOIS (*Rupicapra tragus*).—*From* Buxton's "Short Stalks."

to the probability of its being the ancestor of the domestic breeds. The most interesting of all is, however, the seventh chapter, which treats of the Pyrenean wild goat. Mr. Buxton says that, in contradiction to the true Ibex, this animal largely frequents scrub-jungle, and he points out—we believe for the first time—that the inward inclination of the tips of its horns is evidently adapted to passing with ease among bushes, the true Ibex, which inhabit open country, having the tips of their horns divergent.

Other chapters, relating to Reindeer and Bear, do not call for special notice, and we accordingly conclude by congratulating both author and publisher on the production of a work which, while written professedly for the sportsman, contains much that is deserving of the best attention of the naturalist. R. L.

Our Country's Birds, and How to Know Them. By W. J. Gordon. Crown 8vo. Pp. viii., 152. Illustrated. London: Day & Son, 1892. Price 6s.

Mr. W. J. Gordon is already well known as the author of "Our Country's Flowers," which has attained a well-merited reputation as a handy and convenient guide to the British flora. In the work before us he has treated the birds of Britain in a somewhat similar fashion, the result being a volume which contains, perhaps, more information in a small compass than any other of equal size which has come under our notice. The great feature of Mr. Gordon's work is that every species is illustrated, and that, too, by a coloured figure, and since these figures have been executed by Messrs. Willis and Holding, any comment as to the excellence of their execution would be superfluous. Mr. Gordon, we are glad to see, rather ridicules the idea of including such birds as the Flamingo under the title of British, but as the species has occurred in the British islands, he is perforce compelled to include it in his list.

The great object of the volume is to enable any person to identify any bird he may happen to come across in the British Islands, and for this purpose Mr. Gordon provides us with an elaborately worked-out system of "keys." He is, however, careful to add that this is merely an empirical method, and has nothing to do with classification. Other chapters deal, however, fully with the classificatory portion of the subject, in the course of which the peculiarities of the bones of the skull in the different groups, as well as the subject of pterylosis, are briefly but carefully treated. An unique feature in the book is the series of tables of dimensions of all the British species.

We may point out that in including *Pandion* among the Falconidæ the author is not up to date, we ourselves being persuaded of the correctness of the view that this genus represents an order connecting the Accipitres with the Striges. We cannot, moreover, accept the inclusion of all the Passerines in a single family "Passeridæ."

These, however, are comparatively small defects, and the author is to be congratulated on having given such a large amount of information on British birds in such an exceedingly small compass.

R. L.

The Building of the British Isles: A Study in Geographical Evolution. By A. J. Jukes-Browne, B.A., F.G.S. Second edition. Pp. xiv., 465. 8vo. London: George Bell & Sons, 1892. Price 7s. 6d.

One object of geological enquiry is to picture the physical conditions that attended the deposition of the many geological formations. In its most interesting aspect, the subject is inseparably connected with

the natural history of successive periods; but from this broad point of view all sorts and conditions of life and of land and water would have to be considered.

In the present work the author endeavours to picture the changes in the distribution of land and water, over the area now occupied by the British Islands, during each of the great epochs of time which make up our geological sequence. His task is a most difficult one, for few geologists have been tempted to draw boundary-lines to mark the former limits of formations, however well they may be acquainted with them. It is one thing to picture in the imagination former general geological conditions; it is quite another thing to draw the outlines of land and water and to fill in conjectural rivers and lakes. The subject, like discussions on theology, is apt to lead to almost endless diversity of opinion, for (as the author admits) the materials for forming a definite judgment are so imperfect.

That his work is appreciated, is shown by the issue of a second edition, in which the author has added over a hundred pages of further information. Altogether there are fifteen plates drawn to show the geography of different epochs. Three of these are new, six remain the same as in the first edition, and six have been subject to alteration. Several other small maps and sections are printed with the text. One of the principal alterations is in the map of the Ordovician period (Arenig Epoch); we notice also that a small lake is introduced into the Devonshire and Cornish area, in the map that represents the Permian period.

Whatever value the maps may possess, there can be no doubt that the author has brought together, in a most painstaking way, a large amount of material bearing on the physical geography of our bygone ages. He concludes with a chapter on the Supposed Permanence of Continents and Oceans, and with an appendix on the Sub-oceanic Crust.

LES ALPES FRANÇAIS, LES MONTAGNES, LES EAUX, LES GLACIERS, LES PHÉNOMÈNES DE L'ATMOSPHÈRE. [Bibliothèque Scientifique Contemporaine.] By Albert Falsan. Pp. 288. Illustrated. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

IN a little volume of 280 pages, Monsieur Falsan has given a readable account of the French Alps, their geology, physical geography, and meteorology. The author's extensive experience and well-known writings will attest his competence to speak on the subject, and we need only add that the condensation from larger works has been carefully and judiciously made. The nature of Monsieur Falsan's special studies leads to marked prominence being given to the glaciers and ancient glacial phenomena, and to the Tertiary geology of the Alps. The chapters on these subjects are particularly good, and to them the geologist will naturally turn.

A CATALOGUE OF BRITISH JURASSIC GASTEROPODA. By W. H. Hudleston, F.R.S., and Edward Wilson, F.G.S. Pp. xxxiv., 148. London: Dulau and Co., 1892. Price 7s. 6d.

THIS work, although it embraces a comparatively small section of our British Fossils, is a most important addition to that series which we hope will eventually replace the famous Morris' Catalogue. Nearly forty years have elapsed since the second edition of Morris' work was published. In that volume only five species of Gasteropoda are

quoted from the Lias; now 319 species are enumerated. Altogether, the authors of the present Catalogue record 1,015 species, and they have reduced the lists by something like 150 names of doubtfully identified foreign or imperfectly-constituted British species.

This new volume is the second contribution to Catalogues of British Fossils, Messrs. Woodward and Sherborn's Vertebrata appearing in 1890. We constantly use the old classic now, and considering the vast amount of work that has been done in the past forty years, it will readily be understood how useful must be a recent book of a similar kind that fully arranges our present knowledge. It is gratifying to find that the labour has so far been undertaken by those specially qualified to do it.

We should like, however, to make some remarks on a few points which might, in our opinion, have been more carefully studied. In the first place, the authors do not distinguish between James Sowerby and James de Carle Sowerby, a careless practice of some English palæontologists which has several times been pointed out to us by continental workers. Secondly, there is a great inconsistency in giving dates; sometimes a separate work is quoted with and sometimes without date, and the same may be said of serials. Considering the authors do not attempt exact dating in their bibliography, the reader is left hopelessly in the lurch, and has after all to seek his reference for himself in some public library.

We are glad to see that when quoting pre-Linnean genera, the first Linnean authority is also given, though from the method of quotation one is almost led to believe that the pre-Linnean author's name is accepted. The entry *Cerithium*, for instance, would be more accurately expressed thus: *Cerithium*, Bruguière, 1792 (*ex* Adanson, 1757). It is also gratifying to note that the 10th and not the 12th edition of Linnæus' "Systema" is used.

With regard to the generic determination of some of the mollusca, we think that a more detailed study of recent forms and recent continental work upon them would have enabled the authors to more exactly arrange the various Cerithiform, Naticiform, and Trochiform groups; and although the authors have some remarks (pp. 18—23) on several genera, we are loth to believe that all the "species" quoted under *Cerithium*, *Chemnitzia*, *Natica*, *Trochus* and *Turbo* properly belong to these genera. We are especially dubious as regards the first, third, and fourth genus, in which the characteristic features can only be seen in perfectly-preserved or completely disentombed specimens.

The sundry new names used in the volume might have been listed on p. xxxiv., now a blank; as it is we are told to "find six faces" in the style of the puzzle-pictures.

The general get-up is good, but the paper is too thick and clumsy, and the book takes up one inch, instead of half-an-inch, of already seriously overcrowded shelves.

Fossil Plants as Tests of Climate. By A. C. Seward, M.A., F.G.S. [Sedgwick Prize Essay for 1892.] Pp. 151. London: C. J. Clay & Sons, 1892. Price 5s.

This Sedgwick Prize Essay is a useful compilation, the author having brought together all the botanical evidence of former climatic conditions. Most of the opinions quoted are, however, extremely speculative, and of little value, and, wisely, Mr. Seward seldom ventures an opinion of his own on so difficult a subject as bygone climatic changes.

As far back as plants are known, even in the problematical *Nematophyeus* of the Silurian, rings of growth are visible in certain orders, but whether these are true "annual rings" we have no means of telling. When we study the evidence derived from fossil floras found within the Arctic regions, we are on safer ground, for the Devonian, Carboniferous, Jurassic, Cretaceous, and Lower Tertiary plants could not possibly have lived there under present climatic conditions. Still clearer is the evidence when we come to more recent times, and find alternating deposits containing northern and southern plants of existing species.

LES LICHENS. By A. Acloque. [Bibliothèque Scientifique Contemporaine.] Pp. viii., 376. Illustrated. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

THE French people are happy in the possession of a popular scientific literature of excellent quality and moderate price, of which they are justly to be envied. They produce books now and then, however, of which no nation envies them, and this is one. In the matter of bad popular books on fungi and lichens this country might be thoughtlessly considered to hold the record, but it is refreshing to know that while our neighbours can beat us easily in serious research on these subjects, they can also show us the way in the matter of poor productions. It is almost enough to say of this little popular book on the study of lichens that its benighted author rejects Schwendener's great discovery of the dual nature of lichens, and the symbiotic phenomena they illustrate. It is hard to do more than prove a matter, and after the establishment of this great fact by the researches of De Bary, Schwendener, Stahl, Bornet, and others, it was confidently expected that the old lichenological school would die out. It is dying, however, like a vested interest, and the patent rights in "homæogonidisme" have been leased afresh by M. Acloque. His little book shows a most superficial knowledge of morphology in general, and the text is not much better than the figures, which are all that is not to be desired.

NAKED-EYE BOTANY. By F. E. Kitchener, M.A., F.L.S. [Beginners' Text-Books of Science.] Small 8vo. Pp. xii. and 182. London: Percival & Co., 1892. Price 2s. 6d. net.

THE series of elementary science text-books of which this is the first, has been projected to meet the want, long-felt, of something which will cover the ground of a few terms' lectures and serve the pupils as a supplement to those lectures. The volume comprises 48 lessons, which may "be got up by the pupil by himself as prepared work," or "taken in school unprepared with the help of a running commentary from the master in the form of a catechetical lecture." These two objects are widely different, and demand very different treatment; with the help of a "running commentary" from a competent master the work before us may be of some use, but it is not one to put into the hands of a beginner to "get up" alone. As lecture-skeletons on various types of flowers, the book may be a help to busy teachers, but they must be careful to explain crude statements and correct erroneous ones, such, for instance, as "the leaf-stalk may be traced as carried on through the leaf to its apex, and sends out branches (or veins) on each side arranged like a feather," the definition

of stipules as "two small leaves at the base of the leaf-stalk," or the description of the position of the leaves of the iris "at the root (radical)." Knowing the very intimate connection between respiration and life, whether of plant or animal, it is surprising to find this important function of the whole plant disposed of in a few lines thus: "In the absorption of oxygen from the air, it is not so much the foliage leaves that take part as the leaves that are not green, the parts of the plant that are in flower; these plants perform, when not in the sunlight, a process akin to that which all animals perform, viz., they take in oxygen gas dissolved in water, and send out the oxygen combined with carbon under the form of carbon dioxide dissolved in water."

We know that respiration is more vigorous in opening flowers and germinating seeds, but this is only because life, expressed as more rapid growth and demanding a co-ordinated supply of energy, is "faster"; just as a man breathes harder with increased effort.

It is only fair to state that these faults preponderate in the earlier lessons; where the author confines himself to descriptions of plants he is more at home, and many of the lessons, *e.g.*, that on the Rose-family, are good.

In spite of the title "Naked Eye Botany," the pupil, as stated in the preface, will find a lens useful, and often indispensable, in the floral dissections.

At the end of each lesson problems are presented to be done out of school. Many are good and suggestive, some hopelessly beyond the pupil. At the end of the first lesson, one on the chickweed, he is asked whether "animals move about and plants are fixed in the ground" is an invariable rule, and invited to mention exceptions; several relating to insect visits and fertilisation are introduced with hardly any previous instruction on the subject.

In the table for determining the natural orders of British flowering plants, no notice is taken of exceptions, so that *Clematis*, *Anemone* and other apetalous Polypetalæ would be sought for among the Apetalæ. We notice, too, that both the goose-foot and polygonum family are styled Polygonaceæ.

The book is no worse than many of its kind; better than some. It illustrates for the hundredth time the fact that it takes the best men in a subject to write a good elementary text-book.

Systematic botanists, and those who make a hobby of the Iris family, will welcome the appearance of Mr. J. G. Baker's monograph on Irideæ (George Bell & Sons). It is uniform with the author's works on the Fern allies, the Amaryllidaceæ and Bromeliaceæ. Mr. Baker speaks of it as the last of the series, but we hope this will not be the case; the Scitamineæ want working up again, for Horaninow's monograph might well give way to a more modern one, and if Mr. Baker took it up we should be sure of having it well done.

Professor Baillon's *Histoire des plantes* (Paris: Hachette & Co.) has reached the twelfth volume, the first part of which, just to hand, includes the Conifers and other Gymnosperms, with the monocotyledonous orders, Alismaceæ, Najadaceæ, Typhaceæ, the curious little tropical Triurideæ, and the equally strange, almost exclusive, Australian Centrolepideæ. The excellence of the previous volumes

is maintained both in the text and illustrations. We notice that *Casuarina* now figures as a tribe of Conifers, though with a query, and the remark that wherever placed they constitute a distinct group, which has been compared on as good grounds with the Cryptogams. We regret that the author has followed the change of nomenclature recently introduced by Kuntze, which substitutes *Podocarpus*, the accepted name of a large and well-known genus, for the equally well-known, but very different, *Phyllocladus*, makes the *Sequoia* an *Arthrotaxis*, and for *Welwitschia* writes *Tumboa*.

DR. P. L. SCLATER has issued a privately-printed *New List of Chilian Birds*, compiled by the late Harry Berkeley James. Some time ago Messrs. Sclater and James agreed to co-operate in the preparation of a work on the Birds of Chili, as a companion volume to Sclater and Hudson's *Argentine Ornithology*. The lamented death of Mr. James last July arrested progress, and his preliminary list of birds, compiled as a basis for the work, is now published by Dr. Sclater, and prefaced by a short biographical notice.

MESSRS. GEORGE PHILIP & SON'S *Child-Life Almanac* (price 1s.) will interest not only children, but nature-lovers of all ages. The aim is to provide kindergarten teachers with suggestions for lessons and observations. The recurring facts of natural history, and the principal events in the social, political, and intellectual life of the country, will serve as pegs on which to hang instruction. The idea is well carried out. The twelve sheets, each 11 × 8 inches, record not only a goodly number of events of common or special interest, but also in a parallel column, a list of phenological observations, the times of appearing of flowers, birds, and insects, of the singing or departure of the birds, the leafing or leaf-fall of our common trees, and many other facts which should be of interest. Hints are also given as to out-of-door work available for the time of year, and general observations on pond-life, moth-hunting, bees, or gardening.

Finally, two prizes of a guinea, one for adults and one for children, are offered for the most complete set of phenological notes for 1893, prepared from actual observation, confirming or supplementing those of the almanac. We hope Messrs. Philip will receive a goodly number of sets. Efforts to increase the interest in the study of Nature deserve support. The almanac is very neatly arranged and printed in a somewhat effective cover.

OBITUARY.

HENRY TIBBETS STAINTON.

BORN AUGUST 13, 1822. DIED DECEMBER 2, 1892.

WE regret to record the death of this eminent entomologist, an acknowledged leader in the science at home and abroad. Mr. Stainton was one of the highest authorities on the Lepidoptera, especially on the smaller moths, and most particularly on the Tineina, a group in knowledge of which he had been, of late years, without a rival. Like most naturalists, Mr. Stainton had a taste for his subject from his early youth, as he began to collect insects when only twelve years old. He continued faithful to the Lepidoptera throughout his life; and though the groups to which he mainly devoted his attention do not attract the ordinary amateur, their study is of great importance, and the life-histories of many of the Tineina are of surpassing interest. In the study of these minute moths, Mr. Stainton was associated with the great German lepidopterist, Zeller. By their labours the classification of the Microlepidoptera was brought into a natural sequence, and the genera established on good structural characters, a consummation still to be wished for in some of the groups of larger moths. The *Natural History of the Tineina* in four languages was the joint work of Stainton, Zeller, Douglas, and Frey, and appeared in thirteen volumes between 1855 and 1873. Mr. Stainton had published a work on *British Tineina* in 1854; in 1857 he published the *Tineina of Syria and Asia Minor*, and, in 1869, the *Tineina of Southern Europe*. In numerous papers in the entomological journals he elucidated the life-histories of the smaller British moths, and made many additions to our fauna. But his general work on the *British Lepidoptera*, published in two volumes (1857–9), shows his acquaintance with the order as a whole, and is still the most useful book on the subject for the working entomologist; though being mostly composed of synoptical tables and technical descriptions, with numerous abbreviations, it is not attractive to the general reader.

Mr. Stainton will be remembered for his activity in the field of entomological journalism. For twenty years, from 1855 to 1875, he issued his *Entomologists' Annual*; and for ten years, from 1856 to 1866, he conducted the *Entomologists' Weekly Intelligencer*, the only British journal on the subject that ever appeared weekly. From 1864 till his death, he was on the editorial staff of the *Entomologists' Monthly*

Magazine, and until last year he was a frequent contributor to its pages. He was a member of many learned societies, and was elected F.R.S., a rare honour for a systematic zoologist, in 1867. Courteous to all who applied to him for information, personally or by letter, and always ready to place his great knowledge and extensive collections at the service of students, he leaves an example of the best traditions of English naturalists.

Among other recent deaths we also have to chronicle those of Flaxman Spurrell, of Crayford, an ardent botanist, who also began the great collection of Pleistocene Mammalia from the Thames Valley, now long utilised and extended by his son, Mr. F. C. J. Spurrell; of W. Mattieu Williams, the well-known author of many popular treatises and magazine articles; of the veteran conchologist, P. M. A. Morelet; of two cryptogamic botanists, Baron Felix von Thümen and C. M. Gottsche, both distinguished for their labours among mosses and hepaticæ; and of Alexander Skofitz, founder and editor of the Austrian *Botanische Zeitschrift*.

The death is announced of the veteran American Professor of Geology, Dr. John Strong Newberry. We hope to give some account of his life and work next month.

The death of Sir Richard Owen, K.C.B., on December 18, 1892, is noticed on p. 17.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Dr. G. von Lagerheim, late Director of the Botanic Garden of Quito, has been appointed Curator of the Tromsö Museum, Norway.

Messrs. W. T. Swingle and W. J. Webber have been appointed to direct the newly-founded "Station for Citrous Diseases" at Eustis, Florida.

Mr. E. W. MacBride, M.A., has been appointed Demonstrator in Animal Morphology in the University of Cambridge.

Dr. C. S. Minot has been appointed Professor of Histology and Embryology in Harvard Medical College; and Dr. Harris H. Wilder has been elected to the Chair of Biology in Smith College, Northampton, Mass.

Mr. Joseph E. Carne, late Curator of the Mining and Geological Museum, Sydney, has been appointed a Geological Surveyor in the Department of Mines, New South Wales.

Mr. Edgar R. Waite, for some years Curator of the Leeds Philosophical Society's Museum, and joint editor of *The Naturalist*, has been appointed assistant-curator of the Australian Museum, Sydney. Mr. Waite leaves England early in February.

Dr. F. Elfving has been appointed Professor of Botany in the University of Helsingfors, Finland; and Professor W. R. Dudley has left Cornell University for the Leland Stanford Junior University, Palo Alto, California. Mr. G. F. Atkinson now becomes Assistant Professor of Cryptogamic Botany at Cornell.

Mr. Francis Darwin, Reader in Botany at the University of Cambridge, has been appointed Deputy-Professor of Botany, owing to the ill-health of Professor Babington. Professor Babington has held the post for more than thirty years, and for some time past has taken no active part in the management of the School of Botany, which was so well worked up by Dr. Vines, now Professor at Oxford. Mr. Francis Darwin is well known, both from association with his father in much of the great naturalist's botanical work, especially the "Movement of Plants," and from his own researches in the physiology and biology of plants.

The foundation stone of an extension of the buildings of the Durham College of Science, Newcastle-on-Tyne, was laid by the Earl of Durham last month.

UNDER the direction of Professor Osborn, a series of lectures on the results of current researches in Biology is being delivered this session at the Columbia College, New York. The course is arranged to meet the requirements of advanced students and biologists engaged in research.

ACCORDING to the *American Naturalist* the new Natural History Institute of the University of Illinois, at Champaign, was opened on November 16 last. The cost of the building was about 78,000 dollars, and it will contain the Natural History library (of 20,000 volumes), museum, laboratories, and lecture rooms.

THE Town Council of Ipswich has decided to open the Museum and Public Library on Sunday afternoons.

WE are glad to learn that the Yorkshire Philosophical Society has reduced the admission fee for the York Museum on Saturday afternoons to the small sum of one penny. This generous concession to the public is much appreciated.

THE shareholders of the Bristol Museum and Library have now confirmed their resolution, to offer their property as a gift to the City of Bristol. The discussion of the subject seems to have been chiefly confined to the library, but we hope that some naturalist of influence will be found to watch the interests of the museum. Fortunately, the valuable collections are safeguarded by the endowment fund, which necessitates the interference of the trustees of the British Museum, and their sanction to whatever arrangements may be made.

A FUND for the benefit of the widow and daughter of the late Mr. T. J. Moore, formerly curator of the Liverpool Museum, is being collected by a local committee, of which the Rev. H. H. Higgins is treasurer, and Mr. Richard Paden secretary. Contributions addressed to either of these gentlemen at the Liverpool Museum will be gladly received.

THE plans of the Sedgwick Geological Museum were under discussion at Cambridge last month, and there now seems to be a prospect of the early realisation of the long-projected scheme for a new Memorial Museum. The staff of the Geological Department of the University is also to be increased by the addition of a Demonstrator in Palæozoology.

A HANDBOOK for the Department of Geology in the United States National Museum is in course of publication. Part i., by Mr. George P. Merrill, deals with the Materials of the Earth's Crust, and forms not only a useful guide to the Rock-collection, but also a concise introduction to Petrology. It is illustrated with photographic plates that clearly show various kinds of rock-structure, as seen both in mass and under the microscope.

THE Right Hon. John Morley was elected a Fellow of the Royal Society of London on December 15.

THE Royal Geographical Society's monthly Proceedings appear this month and henceforth under the title of *The Geographical Journal*.

THE section of Medicine and Surgery of the French Academy of Sciences some time ago formed a committee to arrange for an address of congratulation to be

presented to M. Pasteur on the occasion of the seventieth anniversary of his birthday. Subscriptions were received, and a gold medal accompanied the address that was presented to the veteran pathologist on December 27.

The 150th anniversary of the foundation of the American Philosophical Society will be celebrated at Philadelphia from May 22 to May 26, 1893.

When is the Catalogue of the Linnean Society's Library destined to appear? It has been promised to the Fellows for several years, and so far there has been no fulfilment. The Linnean Society's Library is so useful to a large body of workers, and the courtesy and helpfulness of the officers and staff are so abundant, that there has been hitherto no public grumbling on the subject. It is to be hoped that an energetic effort will soon be made to bring out the Catalogue, since its absence considerably impairs the efficiency of this branch of the Society's work.

The Yorkshire Naturalists' Union has issued as the seventeenth part of its Transactions another instalment of Mr. J. G. Baker's "North Yorkshire." This is the fifth part of the work, and contains a list of the Flora from *Pyrola* to *Sesleria*. The next part will complete the flowering plants, and commence the mosses and hepatics.

The North Staffordshire Naturalists' Field Club has issued vol. xxvi. of its *Transactions* (1892). The Garner Memorial Medal has now been established, and will be awarded in alternate years for original researches in the Society's district. The Council regrets that funds do not allow of an annual award. The medal will be of silver, or, at the option of the recipient, it may be substituted by a bronze medal and a gift of books.

The last part of the *Proceedings of the Yorkshire Geological and Polytechnic Society* (vol. xii., pt. ii., 1892) contains an important illustrated account of the exploration of a tumulus at Howe Hill, Duggleby, by J. R. Mortimer. Other papers relating to the county are a description of the geology of Grassington and Wensleydale by J. R. Dakyns, notes on Flamborough Drifts by G. W. Lamplugh, and a summary of the Lias and Oolite formations by James W. Davis, the editor. Mr. G. R. Vine has a detailed memoir on some Cretaceous Polyzoa, and Dr. Tempest Anderson contributes some personal observations on the volcanoes of Iceland.

The Palæontographical Society is to be congratulated on having issued its volume of monographs for 1892 within the year to which the subscription relates. The volume contains the final part of Professor Nicholson's monograph on Stromatoporoids, and further instalments of the monographs of Jones and Woodward (Palæozoic Phyllopoda), Hudleston (Jurassic Gasteropoda), Buckman (Lower Oolite Ammonites), and Whidborne (Devonian Fauna of S. England). The plates and illustrations are of the usual high standard, but there is a marked preponderance of illustrations of ammonites; the Society's stock of plates of these fossils long in hand must by this time be almost exhausted. We have on a former occasion referred to the misleading list of "announcements," and regret to find that it still remains unrevised. In the case of several of these monographs announced as "in preparation," we believe the statement is false, and that members of Council are aware of the fact; indeed, the Treasurer of the Society (Mr. R. Etheridge, F.R.S.) authorises us to state that he is *not* preparing the two memoirs placed under his name, and has no intention of doing so. The length of the list of subscribers is gratifying, but the false promises made by the Council are not calculated to retain this support, as soon as the true state of affairs is discovered.

CORRESPONDENCE.

Experimental Evolution.

In the last number or Natural Science, P. C. M. delivers some criticism o my lectures on Experimental Evolution. He is quite right in criticising me when he says that "While royal food may ripen the latent sex of a worker (*re* bees) a different factor has to do with the distinction between male and female." I should have said that *sexuality*, *i.e.*, the development of sex, or the possession of sex, may be determined by food; *sex* is not determined thus, as while unfertilised eggs develop only into drones, fertilised eggs, according to circumstances—and food—develop into queens *or* workers.

But when P. C M. says, concerning the statement that "we possess in the facts of domestication and inheritance a large number of cases of variation, which occurs in every part, due to environment, and transmitted in various degrees," that "if the statement were true there would be no controversy," this is a rather sweeping criticism. There are five distinct propositions in this statement. Does P. C. M. object to all, or only to part of them? Does he deny variation due to cultivation and domestication? does he deny the occurrence of variation in every part of organisms? does he deny only the fact that variation is due to environment? or does he object to its hereditary transmission? I grant that, in ascribing variation to environment only, I would be saying more than I am prepared to assert, as, in fact, the cause or causes of variation are unknown; but environment in its broad sense seems to have something to do with variation, as many facts go to show. P. C. M. may certainly object to part of my statement, but it would be fair, if only towards his readers, if he stated which part he considers as most heretical.

The same critic informs me that Professor Le Conte's classification of the factors of evolution is "curiously inept," and charges me with approval thereof, which surely cannot improve my situation. However, I must decidedly object to a method (?) of criticism (??) which consists in quoting part of a statement and leaving out the remainder. I have merely given Le Conte's list of "what the factors of evolution are, or are supposed to be" (p. 229: there is not much approval in this), and if P. C. M. had taken the trouble to go on with the quotation, and to quote what I say of this list (which, in fact, is a mere enumeration of the principal factors of evolution proposed on different sides, a mere catalogue), he would have seen that I state that "the five factors are not all recognised by the same group of evolutionists," the two first being especially Lamarckian, and that, in my opinion, no definite view concerning the real value of these different factors can be entertained as long as all have not been subjected to the test of experiment.

If I have "approved" of the list, I do not see that I can be considered as having approved of the scientific soundness of the views expressed in it, inasmuch as no one does adhere simultaneously to each of the five views expressed, and I should be the last to do so, since I ask for proofs of *each* and *all*.

P. C. M. ends with a very virtuous and mighty sentence, which, abridged, might be of high moral use in a copy-book; but I fail to see how it applies to myself. I have not the slightest wish to confound anybody, nor am I familiar with the devices of political controversy—which, I perceive, are exceedingly wicked—and while advocating, from the first to the last page, the necessity of submitting theories to the test of experiment, in order to ascertain the exact situation of that swift-footed fugitive, truth, I certainly did not expect to be charged with resorting to methods which are not only unscientific, but, strictly speaking, unfair.

Paris, *November*, 1892. Henry de Varigny.

[DR. DE VARIGNY brings forward the statement that "we possess, in the facts of domestication," &c., in support of his view that Professor Weismann goes too far when he asserts that we have no proof of the direct production of transmissible changes by means of external influences. As used, the sentence amounts to a single statement that we know many cases of variations due to environment being transmitted.

The whole controversy to which Dr. de Varigny is alluding, concerns the statement which he brings up as an argument on one side of the controversy.

In the matter of Le Conte, I am sorry that I attributed to Dr. de Varigny an approval he now disowns. He wrote (p. 229) :—

"What can the methods of experimental transformism be? The only answer to this question is based on the consideration of what the factors of evolution are, or are supposed to be. At the present moment five are usually recognised. I quote from Le Conte's able paper of recent date."

In the two "factors" I quoted in my review, Le Conte runs together the observed facts of phylogenetic variation and phylogenetic decay of disused parts, with the theory that the result of the action of environment is inherited and integrated. A chief object of "experimental transformism" must be to decide on these theories, and any "method" involving preliminary acceptance or rejection of them is valueless.

I do not consider Dr. de Varigny's statements heretical, but if I did it would be of no interest to anyone. I merely showed that they were confused.

P. C. M.]

THE INTERIOR OF THE EARTH.

IN the summary of my late paper in the *Proc. Cambridge Phil. Soc.*, given in "Notes and Comments" of December, there are two points to which I wish to be permitted to refer. It is stated that I have "reinvestigated mathematically" the effects of a tidal yielding of the earth on a tide of short period, according to the canal theory. Professor Darwin had already made that investigation, and there was, consequently, no need to do it over again. But he had left his result in the form of general symbols, and what I have done is to carry his calculation a step further by substituting for the symbols their known astronomical values. It was in this way that I arrived at a conclusion to what extent the ocean tide might be expected to be diminished if the interior of the earth is liquid.

The Reviewer has, however, given my result incorrectly in saying that I have found that the tide, in the case of a liquid interior, "would be about two-fifths of what it would be" in the case of the earth being solid. What I did say was, that it would be *diminished* by two-fifths if the earth was taken as homogeneous, but only by one-fifth when the fact is taken into account that the outer parts of the earth are of half the mean density. This, of course, will leave four-fifths of the height on a solid earth for the height on a liquid earth. For instance, if the height of the tide from highest to lowest on a solid earth were fifty inches, it would still be forty inches if the interior were liquid. Since we do not know what the exact height of the tide would be on a solid earth, it is perfectly possible that the tides actually experienced may be of the height appropriate to a liquid interior, seeing the diminution caused by liquidity to be so small.

On a review of the whole question, it appears to me that what had been previously done by Lord Kelvin and Professor Darwin, and other mathematicians, had been to show that, unless the earth is excessively rigid, the tidal forces must deform it. But it had been *assumed* that, if so deformed, it would carry the water up and down with it, so that the ocean tides would not be noticeable; but the question whether or not this assumption was valid, had never, so far as I know, been brought to the test of numbers until I made the attempt.

O. FISHER.

Harlton, Cambridge, *December* 12, 1892.

THE WALK OF ARTHROPODS.

I WOULD like to supplement my paper on the "Walk of Arthropods" (NAT. SCI., vol. i., p 676) by calling attention to Mr. Dixon's description of his researches, in *Nature*, vol. xlvii., p. 56, in which some additional details are given, not included in the paper reviewed by me. The spider *(Tarentula)* when running on water caused depressions of the surface-film which were projected as dark shadows on the bottom of the vessel. No shadows were cast by the foremost pair of legs, which proves that the spider, like the insects, has a triangular base of support. The walk of centipedes is also described; the legs are said to move in alternate sets of three.

In referring to investigators of the walk of insects, I regret that I omitted to refer to Professors Miall and Denny, who in their book on the cockroach (p. 79) give a short but excellent account of the locomotion of insects.

Dublin, *December*, 1892. G. H. CARPENTER.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, 67–69 *Chancery Lane, London, W.C.*

The PUBLISHERS *require a few copies of the October number (no. 8) of* NATURAL SCIENCE. *Full price, with postage, will be paid for clean copies not folded, addressed to* MACMILLAN & CO., 29 *Bedford Street, Strand, London, W.C., until the demand is supplied.*

PROFESSOR W. A. HERDMAN (University College, Liverpool) contemplates the preparation of a Handbook to the British Marine Fauna, and desires to receive suggestions from naturalists interested in the subject. He will also be glad to hear from specialists who are willing to co-operate with him in the compilation. The Professor proposes that the work should be a "pocket" or "seaside" handbook, which could be used in much the same way as the botanists' "Field Flora." It might comprise four to six small volumes, each dealing with one or two of the large groups. There would be definitions—perhaps with occasional analytical tables or keys—of orders, families, &c., down to and including genera. Abbreviated descriptions of species would be added, with an indication of size, range, and habitat. Simple figures would be given of all structural features of diagnostic value in the various genera and species.

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NOTES AND COMMENTS.

The Age of the Earth.

AMONG the wider problems of Natural Science towards the solution of which contributions have been made during last month, the most striking is that of the Age of the Earth. Mr. Clarence King, the well-known American geologist and explorer, contributes an elaborate article on the subject to the *American Journal of Science* (ser. 3, vol. xlv., pp. 1–20, pls. i., ii.), in which he claims to have advanced Lord Kelvin's method of determining the earth's age to a further order of importance. He discusses the experimental investigations of Dr. Carl Barus on the effect of heat and pressure on certain rocks, and particularly selects the case of diabase, which has a specific gravity approximately equal to the average specific gravity of the earth's crust. In the light of the new facts, he then reconsiders the probable rate of cooling of the earth, rendering more precise the conclusions of Lord Kelvin, which were arrived at on more imperfect data so long ago as 1862. As the result of the detailed discussion, Mr. King concludes that the earth's age probably does not exceed twenty-four millions of years—in fact, that the estimate of the physicists is approximately correct, while that of the geologists is "vaguely vast" and unreasonable.

We have already referred on a former occasion (vol. i., p. 487) to Professor John Young's observations on the possible sources of error in the geological and biological estimates of past time. We feel convinced there is no sure chronometer beyond the realms of physics and astronomy, and even in those spheres the mathematicians begin with so many assumptions that we are often inclined to look with scepticism on the results. It is, however, satisfactory to learn that those who approach the subject from the physical standpoint

are still pursuing experimental investigations, and if the same conclusions can be arrived at by a multiplicity of methods, biologists and geologists will be constrained to acquiesce.

The Influence on Development of Chemical Changes in the Surrounding Fluid.

Curt Herbst, of Zurich, in the *Zeitschrift für Wissenschaftliche Zoologie* (vol. lv., p. 456), has published the results of an interesting series of experiments on the eggs and larvæ of Echinoids. He concludes that sudden changes in the chemical composition of the sea-water produce great changes in the larvæ. But these changes are due, not to the chemical nature of the introduced material, but to changed physical conditions—chiefly to changes of the osmotic action between the water and the larvæ. He hopes that such experiments tend towards an understanding of normal development, and indicate paths towards the remote goal of a casual interpretation of life-history. By changing the chemical conditions, he hoped to find whether the form of a larva at all depended on the composition of the medium in which it grew. His first result is against a chemical, in favour of a physical influence. The form of the larva varies with osmosis within and without the larva. Clearly Curt Herbst—like so many other zoologists—has not abandoned hope of resolving vital actions into their chemical and physical elements.

Vitalism.

At the present time, indeed, there is abundant evidence of a fundamental change in the attitude of biologists. Among zoologists, one often hears it said that morphology is played out. By this it is apparently meant that the obvious problems of microscopic anatomy are done with ; that series of sections have been made of all the more familiar animals and organs, and that three weeks at the seaside with borax-carmine and a rocker microtome, no longer result in an epoch-making paper, containing a hypothetical ancestor, and a brand new pedigree. This is, no doubt, very depressing, but there are still problems in abundance to solve, and we are far from regarding the microtome as obsolete. On the other hand, it is undoubtedly the case that the pristine enthusiasm for microscopic anatomy resulted in a neglect of many other sides of zoology. Examine the scientific journals of from four to twenty years ago, and you shall find hardly a word of that side of biology Professor Lankester has called Bionomics. At the present time, in almost every journal one sees studies on variation, experimental work on development, investigations of living things as alive.

It is, however, among physiologists that the revival of this side of biology is most apparent, and it is among physiologists that the

word "Vitalism" has been born. Some time ago, we were all taught to regard the human body as a machine. In digestion the processes were regarded as simple chemical processes: in assimilation the intestines and the vessels were membranes obeying the laws of osmosis and dialysis. Respiration was a simple interchange of gases under conditions that could be paralleled with glass tubing and physical apparatus. Now, all this is given up. The elements of the machine are living cells, and the protoplasm of the living cells refuses to act as a piece of apparatus, but remains isolated from, unaccountable to, the laws of physics and chemistry. So, says the new physiology you must give up attempts at chemical and physical explanations; not only are they erroneous but fatally misleading. Protoplasm must be studied as a thing of its own kind; its own facts taken by themselves; its own empirical laws sought after.

Tropical Seeds in the Hebrides.

In the December number of the *Annals of Botany* (vol. vi., p. 369), Mr. Hemsley gives an account of a drift-seed of the tropical *Ipomea tuberosa* which reached the Hebrides, probably from the West Indies, by the agency of the Gulf Stream. Only one instance is recorded, but from the fact of its having in Long Island a Gaelic name, signifying Mary's Bean, it would seem that its appearance on their shore is not an extreme rarity.

Physiological Action at a Distance.

In the same number of the *Annals* (p. 373), Professor Léo Errera, of Brussels, communicates an interesting note on the Cause of Physiological Action at a Distance. This term was used by Elfving to explain some phenomena of attraction and repulsion which did not seem to belong to any of the known categories of geotropic, heliotropic, hydrotropic, &c. He found that pieces of iron and, to a less degree, of zinc or aluminium, as well as different organic substances, such as sealing wax or resin, attract the growing sporangium-bearing filaments of the well-known fungus, *Phycomyces nitens*. All other metals tried were inactive, and the filaments of *Phycomyces* itself showed a mutual repulsion.

From careful experiments, Professor Errera concludes that this apparently mysterious action is merely a matter of hydrotropism; hydrotropism (negative or positive) being the bending of a plant-organ towards the point, not where it will find a minimum or maximum of moisture, but where it will, within certain limits, lose (by transpiration) the greatest or least amount of water. Knowing that a surface which emits moisture repels the *Phycomyces* filaments, it seemed probable that moisture-absorbing substances would produce the reverse effect, and attract them; and as iron certainly absorbs aqueous vapour when rusting, its action on the fungus filament might

be a case of hydrotropism. Thus any modification of iron which lessened its capacity for rusting was also found to diminish its attraction on *Phycomyces:* polished steel scarcely attracts, and nickeled steel not at all.

China clay, which is very hygroscopic, attracted energetically, but china showed no attraction. It has been shown that, although both are essentially formed of silica, agate is very hygroscopic, while rock crystal is not, and agate strongly attracts the filament, whereas rock crystal is quite inactive. The strongly hygroscopic sulphuric acid is also strongly attractive; certain moderately hygroscopic bodies, like white soap, which lose or gain moisture, according to the relative dampness of the atmosphere, repel or attract the *Phycomyces* accordingly.

So great, in fact, is this sensibility of *Phycomyces*, that it may be used as a test of the presence of hygroscopic power. Having noticed that camphor distinctly attracted the filaments and thymol did not, the observer was led to anticipate that camphor is hygroscopic, and this, a fact hitherto unknown to chemists, was confirmed by careful weighing.

On the other hand, the roots of higher plants are positively hydrotropic, and, as would be expected if the author's views held good, they were found to bend away from iron instead of being attracted by it.

CRYPTOGAMS.

THE same number of the *Annals of Botany* is also of special interest to the Cryptogamic Botanist. It contains a paper by Mr. Barber on a new fossil Alga which he has placed with *Nematophycus*—on perhaps scarcely sufficient grounds; one on the development of *Champia parvula* by Mr. B. M. Davis, an American phycologist—a capital piece of sound work; Professor Karl Goebel's paper "on the simplest form of moss," read at the British Association; and Professor Johnson on *Stenogramme interrupta*.

ALGÆ.

THE venerable Swedish phycologist, Professor J. G. Agardh, has earned hearty congratulations by the production of the first memoir of what, it is to be hoped, will prove a long series, in succession to the well-known *Till Algernes Systematik*. The *Analecta Algologica* is a part of the *Acta Soc. Physiograph. Lund.*, vol. xxviii., and bears the stamp of careful and critical work on a level with the best which this great systematist has given us.

The wonderful fertility of the Scandinavian school of systematic workers on Algæ (including such contemporaries as Nordstedt, Kjellman, Areschoug) is only paralleled by the past generation of Britons, which gave us Harvey, Greville, Ralfs, &c. Criticism,

sometimes rather malignant, of the labours of such great men as Agardh and Harvey proceeds mostly from Germany, where the complaint is made that they were not minute histologists, and did not proceed on the lines laid down in the most recent book on the *Mikrotechnik* of Botany. Their only productive systematists in this branch of science, Kützing and Rabenhorst, conspicuously failed to reach the standard of Agardh and Harvey in this respect, and the young German school has yet to show that improved instruments will profit them to the extent of producing a single systematist of the first rank—one whom they can place alongside of Thuret and Bornet in France. The first great phycological systematist "made in Germany" will obtain a hearty welcome.

Mr. Bracebridge Wilson, of the Church of England Grammar School at Geelong, Victoria, who has collected many of the new forms described by Agardh, has printed a very useful list of his collection of Algæ. He rivals Mr. George Clifton and other correspondents of Harvey in the palmy days of collecting Australian Algæ, when new forms needed less looking for than in these later times.

The Pliocene Birds of Oregon.

In a recent issue of the *Journal of the Academy of Sciences of Philadelphia* (ser. 2, vol. ix., pp. 389–425, pls. xv.–xvii.), Dr. R. W. Shufeldt contributes an interesting memoir on the fossil bird-remains from the later Pliocene deposits of the Silver Lake district, Oregon. The bird-bones obtained from these deposits are generally more or less nearly perfect, and are in almost all cases sufficiently well preserved to fully justify the author in the determinations he has made. The majority of the forms belong to existing genera and species, and the bird-life of the Oregon lakes in Pliocene times must accordingly have been very similar to that of the present day. Then, as now, great flocks of swans, geese, and ducks frequented the lakes at certain seasons of the year; while cormorants and pelicans lined the shores, and gulls and terns hovered in the air. Grebes also frequented the sedges, and sandpipers and phaleropes coursed along the marge of the waters. Still, however, in spite of this general similarity in the avifauna of the past and present, there were certain types in the former epoch which would be missed now. For instance, there was a ponderous goose, and likewise a swan, both of which are now extinct. More remarkable, however, was the presence of a gigantic cormorant, of even larger size than the recently extinct Pallas's cormorant of Behring Island; and scarcely less so was that of a flamingo, of which numerous characteristic bones are figured. Herons were represented by an extinct species of the type genus; and a similar remark will apply to the group of eagles. More noteworthy, however, is the presence of a grouse believed to belong to an extinct genus; although it must be confessed that it would have

been desirable that the author should have some more satisfactory specimen than a metacarpus on which to make the determination.

Some interesting observations on the phylogeny of birds, in the course of which the author states that he believes the ancestral types of all the groups to have had keel-less sterna, will be read with interest.

Oysters.

Dr. Bashford Dean, who was sent by the United States Fish Commission in 1891 to study the cultivation of the Oyster in Europe and America, has just issued a "Report on the Present Methods of Oyster Culture in France" (*Bull. U.S. Fish Comm.* for 1890, art. 14, pp. 363–388, pls. lxviii.–lxxviii.), and "The Physical and Biological Characteristics of the Natural Oyster Grounds of South Carolina" (*Ibidem*, art. 13, pp. 335–361, pls. lxii.–lxvii.), published at Washington, 1892. The decreasing supply of the home markets, and the need for information as to the means for keeping up the supply in the best and most profitable manner, were the reasons for Dr. Dean's studies.

In the first report quoted above special reference is made to the difficulties which had to be surmounted by the French, and to the very high state of perfection to which the cultivation of the oyster has been brought, especially in the districts of Arcachon and Auray. "Natural difficulties," he says, "have caused the French to study division of labour in the industry; to make, for example, one locality furnish the seed, another to raise the oyster to maturity, a third to flavour or colour it, and sometimes a fourth to prepare it for transport. Under these conditions the growth of the industry has been especially and almost entirely dependent upon the wise action of the Government. The reservation of the natural grounds as State property, and the forbidding of general public dredging, is generally regarded as the keystone of French oyster-culture. These grounds—once exhausted, now flourishing—are regarded as the permanent capital of surrounding areas, whose profits, in the form of seed-oysters, are shared by all alike." The importance of Coste's experiments and deductions is warmly referred to, and the industry has become a source of considerable revenue, both to the State and to the culturist. The report goes into the full details of the culture, and is considerably enriched by numerous reproductions of photographs of the "pashs" taken by the author and others.

In the second paper, Dr. Dean describes the celebrated "oyster flats" of South Carolina, in which the oyster ("cats' tongues" or "raccoons") may be said to grow wild. The flats on which these oysters grow are acres in extent, and have the general appearance of a low coral reef. Half the life of the oyster is spent in the air, and half under the water. As these areas are mainly mud banks, the

details of the lives of these molluscs in this district are particularly interesting. The report deals in the fullest manner with the natural conditions affecting the growth, the nature of the bottom, the food, enemies, &c., of the South Carolinan oyster, gives detailed analyses of the waters in all districts where the oyster lives, and is illustrated and made clearer by the reproductions of photographs. It would be well if our own Government were to secure a number of copies of these reports, and furnish them to the oyster growers of our coasts.

Mr. John Cordeaux, in the *Naturalist* for January, 1893 (p. 5), continues his records of the migration of birds, as observed on our East Coast. From these "Bird-Notes from the Humber District in the Autumn of 1892," we learn that two "great rushes" of migrants occurred on September 20 and 21, and again on October 13 to 16. Both these rushes took place under exactly similar conditions, *i.e.*, with easterly gales. The past autumn was also remarkable for the unusual number of rare or occasional wanderers which turned up in the district.

"There were brave men before Agamemnon," and there have been ornithologists since Gould, but the results of their labours appear to be unknown to a writer who discourses of the Oházal and Shama in last month's *English Illustrated Magazine*, and displays, in his somewhat pretentious article, an astounding ignorance of recent work on these species and their allies. But what can be expected of one who uses the term "hybrid" as if it inevitably connoted sterility? We can only hope that in future his "ornithological researches" will be brought more up to date, in which case he may be mortified by discovering in his paper "little mistakes" quite as serious, if not as diverting, as that which, according to him, caused Linnæus to name the Oházal *Copsychus* (or rather *Gracula*) *saularis*.

In the *Journal of the Anthropological Institute* for August and November, 1892, Mr. John Allen Brown writes on the "Continuity of the Palæolithic and Neolithic Periods." He divides the "stone age" into Eolithic, Palæolithic, Mesolithic, and Neolithic, mainly according to the workmanship of the implements. Workmanship alone is, however, scarcely a satisfactory test of date, in the absence of distinct geological evidence as to the relative age of the specimens. We observe that the whole of the implements figured by Mr. Brown were found on the surface. The gap that exists in this country between Palæolithic and Neolithic does not yet appear to have been satisfactorily bridged.

An interesting contribution to our knowledge of the reproduction of the Foraminifera was made by Mr. J. J. Lister at the meeting of

the Cambridge Philosophical Society on November 14. Mr. H. B. Brady had already described specimens of *Orbitolites*, showing the margin of the disc crowded with young shells, and Mr. Lister was able to extend his observations by studying the soft parts of specimens collected on the Tonga reefs. It now appears that the reproduction of *Orbitolites* takes place by the formation of spores. Each spore contains a nucleus lying in its "primordial chamber." After several rings of chamberlets have been added, a stage is reached at which the nucleus appears to be represented by numbers of irregular, darkly staining masses scattered through the protoplasm of the central part of the disc. In the later stages numbers of oval nuclei are found in the protoplasm, often arranged in pairs, and in favourable preparations they may be seen to be undergoing division.

WE are glad to notice that the New Zealand Government is actively engaged in preventing the total extinction of the rarer plants and animals of the colony. Acting on the advice of Mr. Henry Wright, the Government has arranged for the purchase of Little Barrier or Hauturu Island, near Auckland, which will be kept as a national preserve. This island measures $4\frac{1}{4}$ miles in length by $3\frac{1}{2}$ miles in breadth, and rises in the centre to an elevation of 2,000 ft. It is generally rugged, but there is comparatively flat land at the northern and southern extremities. Even now its flora and fauna is particularly rich and varied, and no more suitable area could have been secured.

Bulletin de L'Herbier Boissier is the title of a new publication in the interests of Systematic Botany, issued under the direction of M. Eugène Autran, Curator of the Boissier Herbarium at Geneva. The bulletin is to contain original articles, notes, &c., will appear irregularly, and form each year an octavo volume, of about 400 pages, with plates. The first part contains a paper, with two plates, on the genera *Achatocarpus* and *Bosia*, and their place in the natural system, by M. Schinz and the editor; also an enumeration of the plants contained in Fascicle V. of "Plantæ Postianæ," by the collector himself, including a description of the new species. The specimens were gathered for the most part in the mountain chains of Amanus and Kurd Dagh, in the north-west corner of Syria. The chain of Amanus is well wooded, and the flora consequently differs considerably from that of the almost bare mountains of Lebanon and Palestine. The whole occupies thirty-two pages.

THE severe frost of the early part of last month gave unusual opportunities for the study of river-ice on the Thames. In the upper part of the tidal waters, where there is no salt, though the rise and fall of the tide is still considerable, the "ice-foot," or ledge along the

shore, was often three or four feet in thickness, in one place it measured five feet. With the rise of the tide many of these masses of spongy and cavernous, gravel-laden ice were detached, often in blocks of sufficient size to support and carry away the largest stones in the river-bank. So the ice-age is not quite over, and many erratics may have again started on their travels during the recent frost.

In the *Proc. Roy. Phys. Soc. Edinburgh*, vol. xi., p. 215, Mr. James Bennie gives a valuable list of the fossils found in the raised sea bottom at Fillyside Bank, near Leith; a deposit described by Hugh Miller so long ago as 1854. The most interesting result of a closer examination is, perhaps, that the change of level does not seem to coincide with any climatic change, like that indicated by the Arctic shells in the raised sea-beds of the Clyde district. The whole of the mollusca from Fillyside, determined by Mr. Andrew Scott, are still living in the neighbourhood; and the same is the case with the associated flowering plants identified by Mr. Clement Reid.

Mr. R. Etheridge, jun., is engaged on a monograph of the Permo-Carboniferous Invertebrata of New South Wales, and the second part, relating to the Echinodermata, Annelida and Crustacea, has just reached this country. In their general aspect the fossils are much like those from corresponding beds in Europe; at the same time, slight differences are perceptible, which have necessitated the erection of new genera and subgenera, and indicate that the separation of a marine Australian province had already begun. The characteristic, however, that first strikes the eye with regard to this fauna is the large size of the individuals; especially is this noticeable with regard to the crinoids. We are glad to learn that Mr. Etheridge is not only completing the present monograph, but is extending his researches to the Silurian Invertebrata of New South Wales, and to the Palæontology of Queensland and New Guinea.

The Devonian rocks have furnished fields for many geological battles. Peace, however, reigned for some years in the North Devon area, until Dr. Hicks, in 1890, renewed the attack on the presumed orderly succession of rocks, found fossils in the Morte Slates, and claimed them to be no part of the Devonian system. The fossils discovered at that time were too obscure for specific identification, but he announces (*Geological Magazine* for January) that the Morte Slates "are now proved by their contained fossils to be of Silurian age." We have yet to wait the particular description of these fossils, but, in the meantime, Dr. Hicks gives examples of Folds and Faults in the Devonian rocks at and near Ilfracombe, and these disturbances (in his opinion) necessitate a different interpretation of the succession of strata from that generally adopted. Instead of being one con-

tinuous series, with a regular dip to the south, he finds the beds to be much folded in several broken troughs, and to be constantly inverted. Older beds thus appear to overlie (conformably) newer strata. He concludes that the realisation of these facts will necessitate, in future, a great reduction in the thickness hitherto given to the Ilfracombe series, and the rearrangement of the fossil zones. Jukes, in 1866, drew prominent attention to the crumplings of the strata, and he then remarked: "From what I saw elsewhere about Ilfracombe and Mortehoe, I believe that, while there is a real general dip to the south throughout the district, this dip is by no means so prevalent as it appears to be, and that the real thickness is accordingly much less than would be at first supposed." (*Quart. Journ. Geol. Soc.*, vol. xxii., p. 357.)

AN excellent portrait of Sir Archibald Geikie, and a memoir of him by M. de Lapparent, appear in *Nature* for January 5. A brief memoir and a portrait of Professor T. Rupert Jones are published in the *Geological Magazine* for January. The biographical notice of Sir Archibald Geikie is one of the strangest misrepresentations that has appeared for some time. We are glad to observe that a writer in the *Daily Chronicle* of January 14 has placed the facts of the case before the public.

ATTENTION is sometimes called to the poor attendance at the meetings of the learned societies, except on occasions when a warm and exciting debate is expected. This is natural enough when the papers to be read are of a detailed character; they may be important, and they will interest a few members, but to the many they must be dry. At the Linnean Society much time is profitably devoted to the explanation of specimens exhibited. It is, however, questionable whether the average attendance at scientific meetings has seriously decreased. Writing in 1821, Leonard Horner says: "I went to the Geological Society, which seems to me to have got into very feeble hands, and to want a great deal of the energy it had in former days." He refers to times when Warburton, Wollaston, and Greenough were among the leading spirits. Horner, who had joined the Society in 1808, and was chosen as one of the secretaries in 1810, was (at the time he writes) living in Edinburgh, so that he only occasionally attended the meetings. Coming to reside again in London in 1827, he must have enjoyed many of the gatherings, when Sedgwick, Fitton, Buckland, Murchison, De la Beche, and Lyell were there; and also the subsequent proceedings, when, "after the meeting, we adjourned to Lord Enniskillen's; Owen, Clift, Buckland, Fitton, Major Clarke, and Edward Bunbury and his brother, and we had Crustacea, carbonised fragments of Costæ of a mammal (probably Bos-broiled boniensis), and much smoke and merriment."[1]

[1] See Memoir of Leonard Horner, by K. M. Lyell, vol. i., p. 192, and vol. ii., p. 44.

The illustration of scientific lectures or papers by means of lantern-slides is becoming fairly general; and it certainly tends to render the meetings of learned societies more instructive as well as more interesting. In this way the physical features of a country or the microscopic structure of a rock, the organisms of sewage or the "extinct monsters" of many geological periods, may be faithfully reproduced on the screen from photographs or original drawings. The Royal Society, the Linnean Society, the Royal and London Institutions, the Geologists' Association, and other bodies in London, have introduced the lantern into their meeting-rooms with marked success; the Geological Society, however, has hitherto held aloof.

There must be a sad lack of originality, combined with a confused sense of the rights of literary property, at the Royal Gardens, Kew. Not very long ago, the Assistant Director was in trouble about the originality of some observations about sugar canes, and now the Director himself has to answer for a remarkable feat in the way of piracy. At least, this is the case according to a letter we have received from Mr. James Britten, Editor of the *Journal of Botany*. It appears that when Miss North died, Mr. Hemsley wrote for the *Journal of Botany* an obituary notice which was published in that *Journal* for 1890 (p. 329). Last year, Miss North's *Recollections of a Happy Life* was published, and the Fifth Edition of the Guide to the North Gallery has recently appeared, with a short biographical account of Miss North, which is officially stated to be compiled from the *Recollections*, and other sources. Mr. Britten writes that, on the contrary, this biography "is taken bodily, and without a word of acknowledgment, from the *Journal of Botany*. . . . The *Recollections* have yielded twelve lines out of five pages; from the 'other sources,' apart from the *Journal of Botany*, not a sentence has been cited."

Two new botanical journals appeared last month. The one is under the direction of the Department of Botany in the University of California, and named *Erythea: a Journal of Botany, West American and General*. The other is a monthly, entitled *The Orchid Review*, published by Messrs. West, Newman & Co., London. The Germans have also issued a new geological monthly, entitled *Zeitschrift für praktische Geologie*.

I.

On some Problems of the Distribution of Marine Animals.

WHEN Professor Hensen started on his Plankton expedition in 1889, to attack the problem of the "metabolism of the ocean" from a new point of view, and with methods other than had been employed hitherto, he wanted, as is well known, to study the organic life in high seas as free from the influence of coasts as possible, so as to obtain a more accurate conception of the distribution of animals and plants, and to procure a large quantity of material to which no exception could be taken from which to draw conclusions. Of course, he does not believe, as some of his opponents would have one think, that he has determined once and for all, by his captures, the contents of the part of the ocean he passed through; he merely intended to see what kind of material occurred at that particular time, and Hensen himself is well aware that such complicated problems as are resting in the bosom of the ocean can only be attacked, and not solved offnand.

The hypothesis upon which he acted was, that the distribution of organic life, if not influenced by the coasts and the everchanging conditions we observe there, must be sufficiently constant to enable us to obtain an accurate idea of the contents of the ocean by fishing in more or less limited areas with the help of accurate nets.

His views were already contested and defended at the time when the question of supplies for the expedition was being discussed, and there were naturalists who, without being quite convinced of Hensen's views as to the uniformity of the marine fauna, were, nevertheless, strong upholders of his plans, saying that such an undertaking as his was worthy of support in any case, as the results, whatever they might be regarding the uniformity of ocean life, would certainly be a large contribution to our knowledge of biological and morphological questions.

So far as I can ascertain from a relatively small part of the rich material which the expedition brought home (the Craspedote Medusæ, which were given to me to work out), this view has, as a matter of fact, proved true. In the bottles entrusted to me, I was able to examine several new and interesting forms, besides many other species not sufficiently characterised at present, and the material,

though small in comparison to some other groups, is rich enough in itself to allow conclusions in faunistic problems.

The problems of the marine fauna are so varied and numerous that we can discuss only a few of them in this place. For convenience, we might divide them into quantitative and qualitative problems. It is about the former that the combat between Haeckel and Hensen is carried on, *i.e.*, about the uniformity of the distribution, and about the foundation which is given by *single* captures to *general* conclusions. It is too early as yet to enter into this question, basing conclusions only on the results of single groups, but it may be affirmed that everything known until now from the different investigators indicates a much more equal composition of the Plankton than was supposed even by Hensen himself. It is certain that the influence of the coasts extends to a considerable distance, that even islands cause modifications, and that currents often change the aspect of the fauna almost at once; but Hensen is the first to acknowledge the importance of all these factors.

Setting aside these quantitative problems, there remain the qualitative ones, namely, the nature of the composition of the fauna with regard to the different species, and the geographical occurrence of the single forms. In other words, *are there in the sea as as on the mainland areas of distribution with characteristic inhabitants, or* (though certainly the coast has zones of life) *are the peculiarly Plankton forms universally distributed in it?* We know for certain that there are forms of life peculiar to the open sea, Plankton animals, *par excellence*, which are cosmopolitan, and which occur both in the Atlantic and in the Pacific or Indian Ocean, and in very different latitudes. At the last Congress of the German Zoological Society, von Graff exhibited such cosmopolitan forms of the group Turbellaria, and von Martens, Spengel, Chun, and others seconded him by relating similar facts in the Molluscs, Tornarias, Siphonophores, &c., and this led to an interesting discussion as to the probable continuity of the two oceans in former ages. Among the Medusæ we meet with species the distinguishing characters of which are so insignificant that we should, without doubt, consider them as belonging to *one* species had they not been found in such different regions. On the other hand, we know some forms of Medusæ which have been found hitherto only in a certain limited district, which occur in this district regularly, but which have never been seen elsewhere.

Without impairing the fact that there are cosmopolitan forms which are as regular inhabitants of the Atlantic as of the Pacific Ocean, it is to be expected *a priori* that we should find in different latitudes at least a different fauna. Since in a different latitude the life-conditions undergo a marked change, and show differences in the temperature, in the movement of the water, in the weather, and so forth, we ought to find a different adaptation and different characters.

Dana was one of the first to distinguish such geographical faunistic provinces in the open sea by drawing thermic (or, as he called them "isocrymal") lines on the map, which did not quite correspond to the lines of geographical latitude. The observations of temperature were made on shore, and the empirical data were not very large, but he has taken into consideration the "modifying principles," currents, &c., and has deduced some good general conceptions for his work on Crustaceans, the distribution of which had led him to his ideas.

As another interesting attempt in this regard, we may mention the account given by Brandt in his monograph of the Radiolarians of the great oceanic currents in the Atlantic, which he supposes to be the most important factors in the distribution of animal life in the ocean. It is remarkable that he considers them as "circle streams," by the direction of which a large amount of pelagic animals are always kept within certain limits, and only very few of them can be carried away by the side branches of the currents to other currents, temperatures, &c., where they probably perish, while the majority remain under their regular life-conditions. But the attempts to divide the ocean into faunistic districts are very few, whether it is that no such limits have been believed to exist at all, or that our empirical knowledge has not been large enough till now to allow conclusions; and we have to wait for the results of many explorations till we can obtain an idea about these complicated relations.

The Plankton Expedition, limited as it was in extent, has given us some insight at least into a certain part of the ocean, and we may draw conclusions from it with comparative security, since the fishing-stations were fairly close together, and since improved methods were employed, such as, for example, the ordinary vertical net going down always to a depth of 400 m. in order to catch the animals which rise and sink to avoid the changing influences of the surface at certain times. The results which I have obtained in the Medusæ have confirmed my conviction that we can distinguish in the ocean certain districts of horizontal distribution.

The Craspedote Medusæ can be divided, as is well known, into *Leptolina* or metagenetic forms, which are derived from a polypoid stage, and *Trachylina*, or forms with a direct (hypogenetic) development through a free swimming planula and actinula stage. As might be expected from the places of capture, chiefly lying in the open sea, where polyps have scarcely any chance of flourishing, the Trachylina form by far the majority of the Plankton Medusæ, and the few Leptolina which have been discovered always show a relation to the coast. It might be interesting to control this advance, as the result of life in the open sea, by a comparison with other groups, which also have a sessile stage in their life-history. The Narcomedusæ among the Trachylina being acknowledged by all compe-

tent observers to be relatively rare animals, we might not be astonished to find that the Trachymedusæ proper, with the families Trachynemidæ, Aglauridæ, and Geryonidæ, form the chief contents of the Medusan hauls. However, the members of these three families participate in the composition of the hauls in a very different manner in the different regions, as well in quantity as in species.

First of all we notice that the common law on the mainland, that the number of the species constituting a fauna increases towards the Equator, obtains also in the sea. In the North Atlantic very often one single species, sometimes in an enormous number of individuals, formed the contents of the Medusan haul, while near the Equator the net always brought different species of Craspedota, sometimes five or more, to the eyes of the naturalists. Besides this general law we notice that we can distinguish limits of distribution, not only for species, but also for whole families; of course, these are not sharp dividing lines, such as the currents furnish for some species, but one can speak of districts where this or that family is predominant. The Aglauridæ were the chief constituents in the northern part, the Trachynemidæ in the middle, and the Geryonidæ in the equatorial part of the course followed by the ship; we find Aglauridæ and Trachynemidæ in the equatorial district too, but in much inferior quantity and variety of forms, so that we are fully entitled to look upon the Geryonidæ as inhabitants of the warmer oceans. It is in correspondence with this fact that Geryonidæ have not been found hitherto in the North Sea, and that only one example is known from the Atlantic coast of England, whereas they are abundant in the Mediterranean, and are well known to the investigator of embryology there.

The Trachynemidæ cannot be called a tropical and subtropical family with as much right, since their abundance in these districts is not so great; however, they do not seem to pass beyond a certain northern limit, Florida and the Gulf Stream. Haeckel has already recorded as a strange fact that, in spite of so many explorations, no Trachynemidæ have been seen in the North Atlantic. We may confirm these statements by the results of the expedition for the *typical* relatives of this family; however, three somewhat *aberrant* forms have been found in the northern part of the course of the expedition, which probably come from greater depths, and which occur nowhere else, and claim special morphological interest.

A picture of the distribution of whole families can only be a rough one, and to get a more precise conception of the fauna we have to consider the single species. Thus we are able to divide the ocean traversed by the expedition into different regions, with their characteristic or "leading forms." First, we can distinguish a *northern district*, beginning at the Scottish coast, and having the Gulf Stream and the Azores as its southern limits. The characteristic forms here are *Aglantha digitalis*, *Solmaris multilobata*, and *Homoionema* (n. gen.), all

of which never occur in any succeeding part of the course. To the south of the Gulf Stream the composition of the Medusa Plankton changes at once, and we meet with another fauna, which remains the same in its chief components till we pass into the North Equatorial Current, which the expedition passed near the Cape Verde Islands to the west of Africa, so that we might speak of a *second district*. The leading forms in it are *Liriope cerasiformis*, *Rhopolonema velatum*, and *Aglaura hemistoma*. However, this district is not quite uniform; in its western part, from the Gulf Stream to the Bermudas, and to the Sargasso Sea, we find, besides the three forms mentioned above, several other characteristic species, which occur also in the Guinea, and even in the South Equatorial Current, and which can only have come there by the connection of the "circle currents"; while another part of this second district, from the Sargasso Sea to the North Equatorial Current, shows only the three leading forms in astonishing equality. In a *third district*, namely, from the North Equatorial Current through the Guinea Stream to the South Equatorial Current (to the north of Ascension) the fauna is not such a definite one, as we have forms there which occur in the second district also, together with forms which we find only in these southern currents.

From Ascension, Hensen's expedition crossed the Atlantic to Brazil (mouth of the river Amazon), and it is remarkable that in the eastern part of this course some species appear which were not to be found in the western part. Probably they are inhabitants of a colder current coming from, and turning to, the southern temperate zone, and are not able to flourish in the warm water of the equatorial currents. On the other hand, we find in the western half some characteristic forms, as, for example, *Liriope catharinensis*, which occur only in this limited district. From Brazil homewards the ship crossed the third and second districts again, and the same species were found at the corresponding places. Thus we see positively that certain species are confined to certain districts, and we are entitled to speak of the existence of faunistic regions in the ocean. The temperature seems to be an important factor for producing such limits, but what is of still greater influence in determining them is the course of the great oceanic currents, as is quite evident from a comparison of the map with the facts mentioned above. By their effect, also, the wide distribution of other forms, as *Aglaura hemistoma*, which occurs in the whole Atlantic to the north of the Gulf Stream, can be explained. A further interesting fact is the identity of some species of the middle (second) district with the hypogenetic Medusæ of the Mediterranean, and I can state this identity with the greater certainty, since I have studied the Mediterranean fauna throughout a whole year.

In a former publication I made all these statements with great reserve, as being of value in the first place for the Medusæ only, and I laid stress on the fact that especially the division of the ocean into

districts required further confirmation in other groups. Meanwhile, several of the Plankton investigators seem to have come to similar conclusions, and quite recently an interesting paper by Dr. Dahl has appeared, which treats of the different species of the genus *Copilia* (Saphirine Copepods of the open sea) and their share in the composition of the Plankton. Without entering into the quantitative questions, we will compare only the results of this author regarding the qualitative distribution.

First we notice that Dahl distinguishes a difference between a northern region of the course taken by the expedition and a southern one, inasmuch as in the whole district to the north of the Florida Current and the Azores no members of the genus *Copilia* occur; so it appears that the genus *Copilia* is tropical or subtropical, as, for example, the family Geryonidæ among the Medusæ. In the remaining part of the course he found five different species (it is remarkable that both sexes could be recognised, which show, as is well-known, a pronounced dimorphism), and these five species are distributed in a very characteristic manner. Two of them, *C. lata* and *C. vitrea*, appear along the whole course from the Florida Current to Ascension, to Brazil and back to the Azores; they might be regarded as similar to such forms as *Aglaura hemistoma* among the Medusæ. Two other species, *C. mirabilis* and *C. media*, alternate with one another in their occurrence; *media* being found in the Sargasso and in the part northwards of it, *mirabilis* in the southern parts. So these forms seem to substitute each other, while a fifth form, *C. quadrata*, occurs chiefly in the warm currents from the Cape Verde Islands to Ascension. Dahl suggests rightly that this distribution cannot as a matter of fact result from mere haphazard, and he discusses the probable reasons of it, the Atlantic currents and the temperature, in an interesting manner.

If we consider, not the *single* species in their distribution, but the faunistic picture which results from the simultaneous occurrence of several species, we may distinguish for these Copepods the same limits of districts which we have noticed for the Medusæ. If we look at Dahl's chart, we see the abundance of species in the district from the Florida Current to the Bermudas, where some species of the more southern currents occur besides the forms of district No. 2; we see that one district shows the species schematically expressed *a*, *b*, *c*, *d*, another *a*, *d*, *e*, and a third *a*, *c*, *d*, and so on; and we see further that there is a difference between a western part of the Atlantic traversed and an eastern part, all of which results correspond to what we have found in the group of the Medusæ.

Another publication, by Dr. Apstein, deals with pelagic Annelids, the Tomopteridæ and the Alciopidæ. We find that this author, too, can distinguish a northern and a southern region, with the limits drawn above. The Alciopidæ do not occur to the north of

the Gulf Stream and the Azores, whereas the Tomopteridæ are abundant there. He mentions an interesting form, which appears to be cosmopolitan, since it has been found in the China Sea as well; he says that several species also occur in the whole southern region; but for other forms he gives special districts of distribution.

Finally, passing from these groups of animals, we may look forward with interest to the *faunistic results* that will be obtained in other groups. Some preliminary communications of *morphological* interest have also appeared (on the pelagic Anthozoa, by Van Beneden, and on Gastropods, by Simroth), and whatever the results may be regarding the uniform distribution of the Plankton in the open sea, though till now they agree very well with the views of the originator of the expedition, we may be sure that our knowledge of the faunistic distribution and of morphological facts will undergo a remarkable extension by this expedition.

I will not conclude this short sketch without drawing attention to the practical side of such an expedition, and its value for fishery questions in general. Though it may not appear so at first sight, yet, however any group of the animal kingdom may be related to the other groups, and however far apart some families, classes, or types may stand in the morphological system, they are nevertheless in direct physiological connection. The fishery commissioners' task must be not only to observe the fishes in their occurrence and their wanderings, and to study their enemies and their food, but also to take into consideration every pelagic animal, and to calculate the complicated mutual relations existing between the single groups.

Thus I can affirm that even the Medusæ have an importance for the fishery questions; not as enemies of the fishes nor as their food, but as competitors. It is well known that small Crustaceans, chiefly Copepods, form the food of most fishes, and the oceanic Medusæ, which are beasts of prey, feed on the same material. These Medusæ are very voracious (one can often find their stomachs filled to bursting with Copepods) and are very bold in their movements; they are well provided for the struggle for life in the open ocean, and since they occur in enormous quantities, they are certainly of some importance from the practical point of view as well.

If we make a little step forwards in the knowledge of the complicated relations in which the inhabitants of the ocean stand to one another, the beast of prey and the animal it feeds on, as well as the plants which form the "primæval food" (Urnahrung); in other words, if we come to some understanding of the "production and the metabolism of the ocean," then a chief object of the expedition will have been attained.[1]

[1] Some of the first publications of Hensen's Expedition, namely, the description of the journey, &c., have now begun to appear.

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OTTO MAAS.

II.

On Pasteur's Method of Inoculation and its Hypothetical Explanation.

"IN Nature's infinite book of Secrecy," the bacillus and kindred organisms form an interesting, and of late a much-read, page. Their very minuteness, and the difficulty of studying them, lends an additional fascination; and they rise into painful importance in the light of the modern view that they are the cause of disease.

Since splenic fever was first attributed to the bacillus by the two French observers, MM. Davainne and Rayer, in 1861, one disease after another has followed, until finally the cause of influenza has revealed itself under the microscope of the investigator. To give a list of diseases now attributed to it, would be to compile a page from a medical treatise.

All this renders the bacillus of paramount interest, yet a still greater interest attaches to the extraordinary pathways to health pointed out by the brilliant researches of Pasteur. I allude to his well-known system of inoculation in order to confer immunity from disease.

The foundation of Pasteur's process is the cultivation of the microbe—bacillus, micrococcus or bacterium—outside the animal body in a suitable medium. Various substances—meat broth, sugar and peptone, Liebig's extract, &c.—are used in which to grow the microbes. These liquids, or solids, must be sterilised—that is to say, heated until all germs which may have existed in them have been destroyed—and afterwards protected from atmospheric germs by plugs of cotton wool. With these precautions it is found possible to obtain pure cultivations of any microbe which may be sown in the medium.

Into a tube or flask, then, containing this medium a drop of blood, or fragment of tissue, from a diseased animal is introduced. The microbe existing in it at once increases in numbers, and soon renders the medium turbid. A drop of this first cultivation introduced under the skin of a healthy animal reproduces the original disease with which it was associated. To obtain a second cultivation, a drop from the first is placed in another portion of the medium, where the microbe increases as it did in the first. Proceeding in this way, a succession of cultivations may be obtained. Now Pasteur

found that if the cultivations succeeded each other at intervals not greater than twenty-four hours, each successive cultivation retained the power of producing disease with as great energy as did the first. If, however, a longer interval was allowed to elapse a remarkable change took place : the disease was produced in a milder form, and, more remarkable still, the animal inoculated with one of the later cultivations was protected from the more deadly disease. Moreover, by prolonging the period between the successive cultivations the power of producing disease became less and less, until it was entirely lost.

In attenuating—as he terms it—the virus of splenic fever, again, Pasteur used heat and exposure to the air, while in other cases he has used the still more remarkable method of making his successive cultivations in the body of some animal.

This is the method originally pursued with the microbe of hydrophobia. A monkey was inoculated with the virus from a mad dog, and the spinal cord of this monkey was afterwards used to inoculate a second, which in its turn furnished matter to inoculate a third, and so on; and Pasteur found that the matter from the spinal cord of the first monkey produced a milder disease than the original virus ; that from the second monkey a still milder one, and so on ; and by continuing the process long enough he could obtain a virus of any degree of mildness desired.

As with the virus attenuated otherwise, inoculation with this gave immunity from the severer forms of the disease. Closely connected with this is the fact that if the virus, attenuated by passing through a series of monkeys, be passed in the same way through a series of rabbits, it regains all its former virulence and reproduces the disease in its original form ; and Pasteur states that he is able to revive the power of the attenuated virus of splenic fever by passing it through a series of guinea-pigs, beginning with one just born and gradually increasing the age ; and that of fowl-cholera by passing it in like manner through canaries, blackbirds, &c.

A later method of attenuating the virus of rabies was the simple exposure of the spinal cord of a rabid rabbit to the influence of dry air in a flask. At the end of about fifteen days it was found that the spinal cord thus exposed had almost entirely lost its virulence. And in preventive inoculation a series of such cords was used, beginning with the oldest and least virulent, and proceeding to the newest and most virulent.

In the case of swine fever, again, Pasteur attenuated the virus by passing it through a series of rabbits.

Various other methods of attenuation have been used. M. Chauveau[1] has shown that it may be effected by compressed oxygen. With a moderate degree of compression he found the growth of the

[1] *Comptes Rendus*, vol. xcviii., pp. 1232—35.

microbes was rendered more vigorous, while excessive pressure killed them altogether; but with great care he found a certain zone of pressure under which the bacillus of splenic fever was modified in such a way that sheep inoculated with it were protected against the disease.

M. A. d'Arsonval,[2] again, claims that cultures may be attenuated, as well as sterilised, by the use of carbonic acid gas under high pressures.

MM. Héricourt and Richet[3] mention three ways in which cultures may be attenuated so as to be suitable for protective inoculation, viz.:—

1. Making the culture in a less suitable medium, (*e.g.*, beef-tea *not* peptonised).
2. Keeping the cultures till they have passed the period of their most vigorous growth.
3. Cultivating above or below the most favourable temperature for growth.

The general principle of attenuation seems to be, to make the cultures under slightly unfavourable conditions, by which the microbes are rendered less vigorous.

It is not, however, so much with these interesting experiments that I am concerned at present as with the explanation offered by Pasteur and others of the way in which these attenuated viruses are supposed to confer immunity from disease.

There are two general views, the first being that the microbe of the "modified cultivation exhausts the soil," that is to say, deprives the blood or tissues of something necessary to the growth of the unmodified form. This is known as the *Exhaustion theory*. The second view is that some product of growth—some chemical secretion of a toxic character—of the modified virus is inimical to the growth of the original. This is the *Antidote theory*.

Bacteria belong to the vegetable kingdom, and both theories have the support of analogy, that is to say, both explanations have been applied to the case of the higher plants. It is well known that after repeated crops of any species of plant on a particular spot, the land seems, as it were, to become tired of it, and the growth is less vigorous. Two explanations are offered: first, that the particular plant has exhausted the soil of something specially needful for its growth—this is the *exhaustion* theory; secondly, that the plant renders the soil unsuitable for another of its own species by polluting it with its excretions—this is the *antidote* theory.

The former is the explanation most generally received, and is the one which has obtained the support of Professor Tyndall, whose exact words may here be quoted:—

"Now contagia are living things, which demand certain elements of life, just as inexorably as trees, or wheat, or barley; and it is not

[2] *Comptes Rendus*, vol. cxii., p. 667. [3] *Comptes Rendus*, vol. cvii., p. 690.

difficult to see that a crop of a given parasite may so far use up a constituent existing in small quantities in the body, but essential to the growth of the parasite, as to render the body unfit for the production of a second crop. The soil is exhausted, and until the lost constituent is restored, the body is protected from any further attack of the same disorder. Such an explanation of non-recurrent diseases naturally presents itself to a thorough believer in the germ theory, and such was the solution which, in reply to a question, I ventured to offer nearly fifteen years ago to an eminent London physician. To exhaust a soil, however, a parasite less vigorous and destructive than the really virulent one may suffice; and if, after having, by means of a feebler organism, exhausted the soil without fatal result, the most highly virulent parasite be introduced into the system it will prove powerless. This, in the language of the germ theory, is the whole secret of vaccination."[4]

Serious objections may be urged against both explanations. Indeed, the difficulties in the theoretical conceptions of how the benefits are supposed to arise are sufficiently great even to raise doubts as to the reality of the benefits themselves.

At the very outset of our enquiry, we are confronted by the fact that these organisms, which are thus supposed to depend for their power of increasing on certain substances secreted in small quantities in the animal body, can yet be grown outside of it in various preparations. The various meat broths, sugar and peptone, extract of beef, yeast water, blood serum, &c., which are used, can scarcely be supposed to contain these substances, which are not essential constituents of the body, since the animal—as in the case of those inoculated for any disease—can live and be perfectly healthy without them.

Whatever, at any rate, may be thought of the others, water of yeast, in which Pasteur cultivated the bacillus of splenic fever, cannot be supposed to do so; and although boiled potato is a medium not generally used for pathogenic—that is, disease-producing—microbes, they have been cultivated on it. Thus, Dr. Klein ("Micro-Organisms and Disease," p. 50) relates that good crops of the micrococcus of pneumonia had been reared on boiled potato. Again (p. 103), he tells us how Groffky grew the bacillus of malignant œdema on potatoes. This, at least, most certainly shows that the growth of the microbe cannot depend on a something which exists only in minute quantities, and as an unessential ingredient in the blood or tissues of an animal.

In these preparations, moreover, the microbes increase not merely until they might be supposed to exhaust the medium of the small quantity of this something, but until they have exhausted it of all its nutritive elements, or have produced enough of their toxic principle to prevent their own further growth.

If this something is a normal constituent, how can the blood be deprived of it without serious injury to health? If, on the other

[4] "Louis Pasteur: his life and labours." Introduction, pp. xxxv. and xxxvi.

hand, it is not a normal constituent, how could it ever be present without injury? And, under the latter supposition, a person in perfectly normal health would receive no benefit from inoculation, since there would be nothing for the modified microbe to remove.

Nor has it been explained how the microbe of the modified cultivation could multiply in the body without producing the same inconvenience as the original form, supposing the microbe is, in itself, the cause of the disease.

If, as has been suggested, the malady is caused by the mere number of the microbes clogging the veins, obstructing the circulation, and taking matter from the blood, then the modified form would accomplish this as readily as the other.

But passages might be quoted which seem to imply Pasteur's belief that the "attenuation" of the virus is due to the loss of power to multiply in the animal body. The following is from the English translation of the life of Pasteur:—

"After having reduced the microbes of fowl, cholera, and splenic fever to all degrees of virulence, and brought them to a point where they could no longer multiply in the bodies of animals inoculated with them, and fixed them in media appropriate to their life, Pasteur asked himself whether it would not be possible to restore to these attenuated microbes—weakened to such a degree as to have lost all virulence—a deadly virulence, and to render them again capable of living and multiplying in the bodies of animals."[5]

Enfeebled in such a way, the microbes could scarcely exhaust the blood of that which would have supported a sufficient number of the unmodified microbe to produce disease.

Again, in the following passage it is distinctly asserted that the lack of virulence is due to the loss of power of living in the animal body:—

"The extraordinary fact is then established that the virulence may be entirely gone while yet the microbe lives. The cultures offer the spectacle of a microbe indefinitely cultivable, yet, on the other hand, incapable of living in the bodies of fowls, and, in consequence, deprived of virulence."[6]

In the modification of the splenic fever microbe the cultivations gradually lose their power of increasing in the artificial medium, and finally refuse to multiply any further; the last sowing does not produce a crop. It is one or more of the cultivations preceding the last which are used as vaccine.

Can this microbe, which has almost lost the power of reproduction, be supposed to be able to exhaust the blood of that which would support an abundant crop of the unmodified form? And if this Scylla of lack of reproductive power be hypothetically avoided, the attempted explanation splits on the Charybdis of abundant reproduction producing all the evil effects of the unmodified virus.

[5] *Op. cit.*, p. 246. [6] *Op. cit.*, p. 226.

On the other hand, if the disease is caused by the production of some toxic principle, we must surely suppose this power of production to be an essential part of the nature of the microbe, and it is difficult to understand how this power could be lost by mere cultivation ; for the fact is insisted on that the modified virus is the same organism—a definite and distinct species of microbe—as the one which produces the disease in its deadly form ; and if it produces the same poison it ought to produce the same disease.

If, again, the microbe produces disease by depriving the blood and tissues of matter essential to life, would not the modified cultivation do the same ? In fact, we may assume that to use up the essential specific matter—"exhaust the soil"—the microbe of the modified cultivation would have to increase as greatly as the unmodified form would require to do to produce the disease ; and so, either by obstructing the circulation by mere numbers, by robbing the organs of their nourishment, or by the production of morbific matter, it would cause the same inconvenience as the original virus.

Leaving these difficulties, what grounds are there for the assumption that there can exist in the blood and tissues a something secreted slowly and in small quantities, and yet not essential to health ? As far as I am aware, there is absolutely no proof of the existence of anything of the sort. According to Pasteur it is secreted continuously, and yet in such small quantities that it requires in some cases years to restore a sufficient quantity to the blood to allow of the free growth of the organism. It seems to me contrary to the general principles of physiology to suppose the blood is continuously elaborating a useless product, which accumulates, and yet causes no harm : it is a direct violation of the much-talked-of "law of parsimony" in nature.

A curious and ingenious suggestion as to what this essential something may be is made by Dr. Maclagen, in an article on "Influenza and Salicin" (*Nineteenth Century*, February, 1892).

The microbe, he says, is a parasite, and every parasite requires, besides the essential elements of growth, its own particular nidus. Each microbe finds its nidus in some special part of the body—the liver, the spleen, the skin, &c., and this nidus, he suggests, may be some now-useless character handed down from some of our remote ancestors :

"This nidus once exhausted, is, as a rule, never replaced, showing that, like our rudimentary tail, it is something which is not really essential to our well-being—like our rudimentary tail, it may be some peculiarity derived from a very remote ancestor."

Once exhausted, this nidus is not replaced, and that, according to Dr. Maclagen, accounts for the immunity which one attack of disease confers from a second.

It does not, however, account for the fact that inoculation requires to be frequently renewed—every year for splenic fever according to Pasteur, and every seven years for small pox according

to the medical faculty; and, like Pasteur's, Dr. Maclagen's explanation does not account for the fact that the microbe can be grown outside the body where the nidus does not exist.

And how is the change in the organism supposed to be brought about? Pasteur believes the attenuation of the virus to be due to the action of the oxygen of the air, except, of course, in those cases where it is passed though a series of animals, or where the change is partly attributed to heat; but it is difficult to understand in what way oxygen could alter the character of a living organism so as to cause it to produce varying effects. Is there any case known of an organism altered in such a way? There are three cases among the organisms we are considering, in which the essential characters of the species are said to have been thus changed by the method of cultivation :—

(1.) Buchner has stated that by successive cultivations he has transformed the bacillus of anthrax into the harmless hay bacillus. He likewise claims to have transformed the hay bacillus into the deadly bacillus of anthrax.

(2.) A bacillus found on the seeds of *Alexus precatorius* (an Indian and South American leguminous plant used in certain diseases of the eye), and apparently identical with the hay bacillus, is, according to Sattler, capable of being transformed into a disease-producing form. An infusion of the seeds, known as jequirity, is made and several cultivations of the bacillus started from this in the ordinary sterilised media. The change in the nature of the bacillus is supposed to be effected by growth in the original jequirity fluid, and to be retained in the successive cultivations, since all these were found to produce severe ophthalmia; and not only did Sattler suppose the original jequirity bacillus was thus modified so as to produce disease, but that germs of other harmless forms which might settle down in the jequirity solution likewise became pathogenic.

(3.) The third case is that of a common mould, *Aspergillus*, which has been found to produce death in rabbits when its spores are introduced into their systems, although not in the proper sense of the term a pathogenic form.

Dr. Klein has carefully examined these cases, and repeated the experiments, with the result that all break down as examples of the change of septic into pathogenic microbes. "I feel sure," he says, "any one might as soon attempt to transform the bulb of the common onion into the bulb of the poisonous colchicum." Other observers have corroborated Klein's conclusions.

Ought we, then, to believe that by a series of cultivations a microbe can lose the power of producing disease which it once possessed?

If the microbes of disease can be altered so as to produce a modified disease, it ought to be possible to produce, say, a lactic acid microbe, which would produce a modified lactic fermentation; and further, such a modified fermentation should protect the liquid from

fermentation by the unmodified ferment. In the same way, it ought to be possible to modify the microbe of putrefaction so as to produce a decay-proof body.

With regard, again, to the other explanation that the modified microbe confers immunity by the secretion of some toxic principle which prevents the growth of the unmodified form, it might be asked:

(1.) Would not the secretion—or production in any way—of such a toxic principle be highly injurious to the animal in which it was secreted?

(2.) Why should a toxic principle be allowed to remain in the system for the year required by Pasteur's view on inoculation for splenic fever, or for the seven years required by the received view on vaccination?

(3.) Would not the mere existence in the blood and tissues of the number of microbes necessary to produce a sufficiency of the toxic principle be in itself enough to produce the unmodified disease?

Dr. Klein ("Micro-organisms and Disease") adopts this "Antidote theory" of inoculation.

But apart from the difficulties I have hinted at above, another arises for Dr. Klein, in the fact that he himself adopts the view that death, in the case of any disease caused by a microbe, is due to the chemical alteration produced in the blood and tissues. That is to say, either a change is produced in the blood analogous to that from sugar to alcohol effected by the yeast-plant, or a special ferment is secreted by the microbe. Thus the same toxic principle which causes death in the disease, is the cause of immunity from the disease when the animal is inoculated.

Now if we suppose the microbe used for inoculating increases as much as the unmodified form, then it ought to produce as great an amount of the toxic principle, and hence cause the disease in as severe a form. If, on the other hand, by increasing less, it produces less of the poison, we cannot suppose there would be sufficient to prevent the growth of the unmodified microbe if introduced; for it is to be remembered that the unmodified microbe is supposed to go on increasing until it has produced sufficient of the poison to prevent its own further growth. This is the explanation of the cessation of the disease. Hence the amount of poison necessary to prevent the growth of the unmodified microbe—in other words, to prevent the disease—is the same as that required to produce the normal type of the same. No amount of the toxic principle less than what would itself produce the disease, can be sufficient to prevent the growth of the unmodified microbe—that is, to prevent the disease.

Thus, then, as it appears to me, neither the "Exhaustion theory" nor the "Antidote theory" explains immunity from disease conferred by inoculation.

An assumption which underlies both explanations is, that after a certain number of generations, the microbes are obtained

absolutely free from all matter derived from the body of the diseased animal; but with regard to all experiments on the subject, it is to be wished it could be more conclusively shown that a more subtle poison than the comparatively gross microbe may not be present. It is assumed that the quantity of anything existing besides the microbes in the original particles of injected matter would be so small that after a few generations nothing could remain; but in connection with this we have to remember the extraordinary divisibility of matter, and also that, conceivably, this other something may have the power of increasing as well as the microbe.

Might it not possibly be that the action of the oxygen—which Pasteur thinks modifies the microbe—gradually destroys this other poison? and might not the phenomena of protective inoculation be an acclimatisation of the system to gradually increasing doses of poison, akin to that by which a man can accustom himself to larger and larger doses of arsenic and opium? But this is the merest suggestion.

It involves, moreover, the larger question of whether the experiments of Pasteur and others really necessitate the belief that the microbe is the sole and primary cause of the disease, and cannot be treated of here.

The above remarks apply to attenuated viruses in which the microbe itself is supposed to be the active agent. But many recent investigators, instead of using an attenuated culture of the microbe for inoculation, inject small quantities of the toxic principle produced by its growth. After the culture has proceeded to a certain extent, it is carefully sterilised, and the toxic principle is obtained free from microbes or their germs. To such methods of inoculation the exhaustion theory, of course, does not apply.

It has even been shown, in the case of the bacillus of tetanus, that it is the toxic principle, and not the microbe, which causes the disease. Thus MM. Vaillard and Vincent[7] assert that if the inoculation be effected with a culture in which the bacillus has not had time to produce the toxic principle, or with a culture further advanced, but washed, so as to get rid of the poison, the symptoms of tetanus are not produced. On the other hand, a very small dose of the toxic principle does produce them.

MM. Rodet and Courmont,[8] again, have shown that in the cultures of *Staphylocoque pyozène* there are two distinct principles, which can be separated by the action of alcohol. The one, insoluble in alcohol, has a prophylactic value; the other, soluble in alcohol, renders an animal more susceptible to the disease produced by the microbe. Thus, while animals inoculated with the former are more or less protected from the disease, those inoculated with the latter are rendered more susceptible to it.

[7] *Comptes Rendus*, vol. cxii., p. 239. [8] *Comptes Rendus*, vol. cxiii., p. 432.

Since there is no proof that the toxic principle is retained in the body—indeed, it has been shown that in a diseased or vaccinated animal the toxic products are eliminated in the urine—immunity conferred by inoculation, if of more than momentary duration, cannot be due to the actual presence of the poison.

M. Bouchard,[9] again, has shown that the toxic products of bacterial growth contain two antagonistic principles, which he has termed *anectasine* and *ectasine*. Possibly these may correspond to the two principles of MM. Rodet and Courmont.

According to Cohnheim, the passage of the white corpuscles of the blood through the vessels (diapedesis) is the dominant phenomenon of inflammation; and, according to MM. Massart and Brodet, these white corpuscles possess a certain chemical irritability, which causes them, when placed in solutions containing the products of bacterial growth, to pass from those parts where the solution is more dilute to those where it is more concentrated. These facts are connected in an interesting manner with inoculation.

Local inflammation is one of its phenomena, and it may be supposed that the attractive action of the toxic principle of the vaccine has caused diapedesis of the leucocytes.

According to M. Bouchard, however, it is only the principle he has named ectasine which promotes diapedesis; the other, anectasine, hinders it, and has been used by him for the prevention of hæmorrhage. When the leucocytes, thus drawn to the seat of local inflammation, meet with any microbes, they envelop and digest them. This is the phenomenon of phagocytosis. It may be held to explain a temporary immunity from disease when subject to infection; for supposing an animal is inoculated with the toxic principle, then the leucocytes are drawn to the seat of inoculation by the attractive force of the ectasine. In their consequent state of activity, and outside their proper vessels, they are in a favourable state for meeting with and destroying, accidentally or purposely, introduced microbes. This would explain immunity from disease existing *immediately after inoculation*, which is, perhaps, all that has really been experimentally proved. But it can scarcely be supposed that this abnormal activity of the leucocytes, and their readiness to attack the microbes, could be retained for any length of time.

It may be further added that Pasteur himself has expressed his conviction that the vaccinal matter of rabies is really a toxic principle produced by the microbe, and that in the case of the spinal cord of the rabbit exposed to heat and dry air, the microbe is destroyed and not modified.

G. W. Bulman.

[9] *Comptes Rendus*, vol. cxiii., p. 624.

III.

The Industries of the Maoris.

DEFINITE and well authenticated details of the habits, customs, or manufactures of rapidly disappearing peoples of the world are exceptionally interesting, not only on account of their intrinsic value, but because of the aid they give in the elucidation of phenomena connected with nations which have totally disappeared, and are only known by the few scattered objects they have left behind. From this point of view, the researches of Colenso[1] and Chapman[2] on the ancient works of the Maoris of New Zealand are valuable. Mr. Colenso has an experience extending over more than half a century, and was personally known to many of the old Maori chiefs. During his early years he had opportunities to become acquainted with many of their works which have long been obsolete, and are "scarcely known even by name to the present generation of Maoris." Nature provided these people with a variety of plants from which flax can be obtained, the principal one being the well-known *Phormium*. The finest flax was used in the weaving of garments or dress mats, the weft and warp were of different sorts of flax, and the extremely soft lustrous appearance was obtained by repeated tanning and the most careful selection of threads of the proper colour. The finest and most beautiful of these dresses are twenty or thirty years old, and it is doubtful if they can be produced at the present time. It is not that the art of weaving is lost, but the taste, skill, and patience in the selection of fibres and their dyeing are no longer to be found among the degenerate Maoris. They also wove floor and bed-mats of flax leaves, cut into narrow lengths and bleached in the sun. Baskets were made of similar materials, and little cots for the first-born child are frequently gems of weaving art made by the mother.

Flax was also used for making cords and threads. "It was ever to me an interesting sight to see an old chief diligently spinning such lines and cords—always done by hand, and on his bare thigh. The

[1] "Vestiges, Reminiscences, Memorabilia of Works, Deeds and Sayings of the Ancient Maoris." By W. Colenso, F.R.S. *Trans. and Proc. New Zealand Inst.*, vol. xxiv., p. 445, 1892.

[2] "On the Working of Greenstone or Nephrite by the Maoris." By F. R. Chapman. *Loc. cit.*, p. 479.

dexterity and rapidity with which he produced his long hanks and coils of twine and cord, keeping them regular, too, as to thickness, was truly wonderful." A great variety of ropes were made to suit special requirements; flat plaited ones to put over the shoulders in carrying loads, thick stranded ones for heavy work, and a peculiar fine cord bound round with a still finer one like the fourth string of a fiddle, used only for one purpose—to bind the under aprons of chiefs' daughters.

The Maoris made fishing nets of enormous size, 5 fathoms deep and two or three hundred fathoms in length. With these mackerel were caught, a fish which has since disappeared almost entirely from the waters round New Zealand. They also caught sharks and fresh-water eels, which were dried and preserved for future use; there was no mutton in those days. Bivalves and cray-fish were also extracted, dried, and preserved. The rat was a great delicacy, once very plentiful; it is now extinct. The following is Mr. Colenso's description of its preparation:—"It was carefully singed and so denuded of its fur, then its bones were broken within the body, and extracted by the anus without breaking the skin; this done it was cooked in their earth-ovens, and being very fat, made choice plump morsels, somewhat resembling large sausages. The contents of its stomach (being a frugivorous animal) were also eaten." The Maories were fond of perfumes which they prepared from Hepaticæ, Hymenophyllum, and other plants; the choicest was got from the gum of the peculiar plant *taramea* (*Aciphylla colensoi*) with much ceremony. Interesting descriptions of the manufacture of black pigment for tattooing made by burning resins and catching the soot are given; the soot is mixed with fats, and must then be eaten by a starved dog, and the voided fæces gathered for use.

The extremely interesting and valuable paper by Mr. Chapman is replete with information of which only a brief abstract can be given. It is forty or fifty years since there was a regular manufacture of stone implements, and too little is known of the way in which these implements were made and used. So soon as the savage acquires a steel axe and a gun, his beautiful but ineffective stone weapon becomes useless; it is laid aside, and no more are made. In a few years all the elder savages are dead, and the younger ones have a very transient impression of the ways of their cannibal ancestry.

With small exception, the whole of the various kinds of *pounamu* or "greenstone" is found in a restricted locality on the west coast of the south island. It occurs in boulders in the deposits of gravel in the beds of the Rivers Taramakau and Arahura, and the boulders are also found on the beach at the mouth of the rivers, cast up by the sea. The location of the dyke or vein from which the boulders came is not known. Formerly the stone was rare and expensive, but since these river gravels have been worked and washed for gold, considerable quantities have been found, and it is now not worth more than

one shilling a pound. The ancient importance attached to this district and its greenstone is indicated by the Maori name Waipounamu, which has given its name to the whole of the island. Pounamu was one of the sons of the great Polynesian deity Tangaroa (Lord of the Ocean), who was the son of Rangi (Heaven) and Papa (Earth). The substance *pounamu* was formerly supposed to be generated inside a fish, a shark, and only to become hard on exposure to the air. It appears to have always occupied a prominent position in the mythology, and to have been intimately associated with the history of the Maoris.

The mode of working and fashioning the *pounamu*, or "greenstone," is variously described. The material was first cut into slabs of the form required; this was done by sawing with thin pieces of slate or other hard material, assisted by sand and water, first on one side then the other, until the required piece could be broken off. The smaller fragments were fashioned into ear pendants by the women and children. "With pretty constant work, a man will get a slab into a rough triangular shape, and about 1½ inches thick, in a month, and, with the aid of some blocks of sharp, sandy-gritted limestone, will work down the faces and edges of it into proper shape in six weeks more. The most difficult part of the work is to drill the hole for the thong in the handle." For this, pieces of sharp flint are obtained, and set in the end of a split stick, being lashed in very neatly, with a stone weight on either side, and forming a sort of teetotum drill; as one flint is used up, another is inserted in its place. The enthusiastic Maori carried his partly-manufactured *mere* about with him, and every halt was utilised for taking a rub at it. The Greenstone was probably worked at all the Maori villages; but some places have the appearance of having been especially engaged in the manufacture; in these, numerous fragments, unfinished objects, as well as finished and polished implements, are found. In one instance, immense numbers were dug up in making a garden. In another, most of the remains are found in *whares* or dwellings; the latter, buried in sand, are indicated by hearth-stones, below the level of which there is usually a receptacle under the floor, probably covered by a flax mat when inhabited. In this secret repository are found beautifully finished objects of greenstone, and, perhaps, pieces of hæmatite; the latter, ground and mixed with sharks' oil, was used to adorn the person of the ancient Maori.

The *mere* or axe was the most famous weapon of the Maoris; it was usually 13 to 15 inches in length, sharpened at one end, with a hole through the handle, through which was a strong thong of dogskin made into a running noose, through which the thumb would slip easily. It was carried in the belt. The *mere* was not used like an axe, there was too great danger of its being broken and the labour of years lost. The first contact of the fighting forces was with the *hani*, a sort of quarterstaff. As the fighting got closer the *mere* was

taken in hand. With the left hand the enemy's hair was grasped, and with the right the *mere* was plunged into the side of the head where the bones are weakest. It is recorded that Te Wherowhero, the father of the chief who afterwards became the Maori king, and is still so called, killed 250 prisoners of war at a sitting, smashing the head of each at a single blow. His son still has the *mere*. The weapons were held in high veneration, and were frequently buried with the chief. The *mere*, as well as other objects, were often held as symbolical of ownership of land. The title deeds of the famous Heretaunga Block, now worth three-quarters of a million, was a small *pounamu* pendant, now worn by a gentleman on his watch chain. In 1856, when the final negotiations were made which secured to England the northern part of the South Island—a district very highly prized by the Maoris as the scene of many hard-fought battles—Ropoama-te-One, after alluding to those wars, struck into the ground, at the feet of the Commissioner, Sir D. McLean, a greenstone axe, saying, "Now that we have for ever launched this land into the sea, we hereby make over to you, as lasting evidence of its surrender, this adze, named Paiwhenua, which we have always highly prized from having regained it in battle after it was used by our enemies to kill two of our most celebrated chiefs, Te Pehi and Pokaitara. Money vanishes and disappears, but this greenstone will endure as durable a witness of our act as the land itself which we have now, under the shining sun of this day, transferred to you for ever."

A large mass of detailed observations is recorded by the authors of the papers; sufficient has been said, however, to show the very interesting character and high value of their communications.

James W. Davis.

IV.

Some Recent Researches on Insect Anatomy.

IN a recent review of Lowne's work on the Anatomy of the Blow-fly in NATURAL SCIENCE (vol. i., p. 551), the question of the morphology of the mouth parts in the sucking insects was discussed.[1] Two noteworthy contributions to the subject have lately been made. Léon (1) describes and figures from a photograph a rudimentary three-jointed labial palp on either side of the base of the rostrum of an undetermined hemipterous insect. It is clear from this that the labial palps do not form any part of the rostrum in the Hemiptera, and Léon supports Gerstfeldt's view that that organ represents the parts of the labium (second pair of maxillæ) except the palps.

Of the mouth-parts of the Diptera, Müggenburg (2) has lately written, describing in detail the proboscis in the remarkable parasitic group generally known as the Pupipara. These insects are distinguished from other Diptera by their metamorphosis, as far as the pupal stage, taking place within the body of the mother; and they were, on this account, long separated as a distinct sub-order. Brauer, however, considered them aberrant and degraded members of the group to which the house-fly and blow-fly belong; his view has been shared by most recent writers on the Diptera, and is supported by Müggenburg's researches. The latter observer describes in detail the mouth organs of *Melophagus* (the sheep-tick) and *Braula* (the bee-louse). In the former insect the proboscis is elongated (Fig. 1) as it is also in *Hippobosca* (the horse-fly), *Anapera* (the bird-louse), and *Lipoptena* (the deer-fly), which all belong to the same family. In *Braula* (Figs. 2, 3) and also in *Nycteribia* (the bat-louse) the mouth-parts are much shortened. In no case are mandibles present. The maxillæ (which it will be remembered are believed by Lowne to form the larger part of the proboscis in the Diptera) are, according to Müggenburg, in the Pupipara represented by two elongated structures (Fig. 1, mx^1), situated within the head capsule, which, moved by a complicated system of muscles, assist in protruding or withdrawing the proboscis to whose base their distal extremities are applied. Maxillary palps are present in both

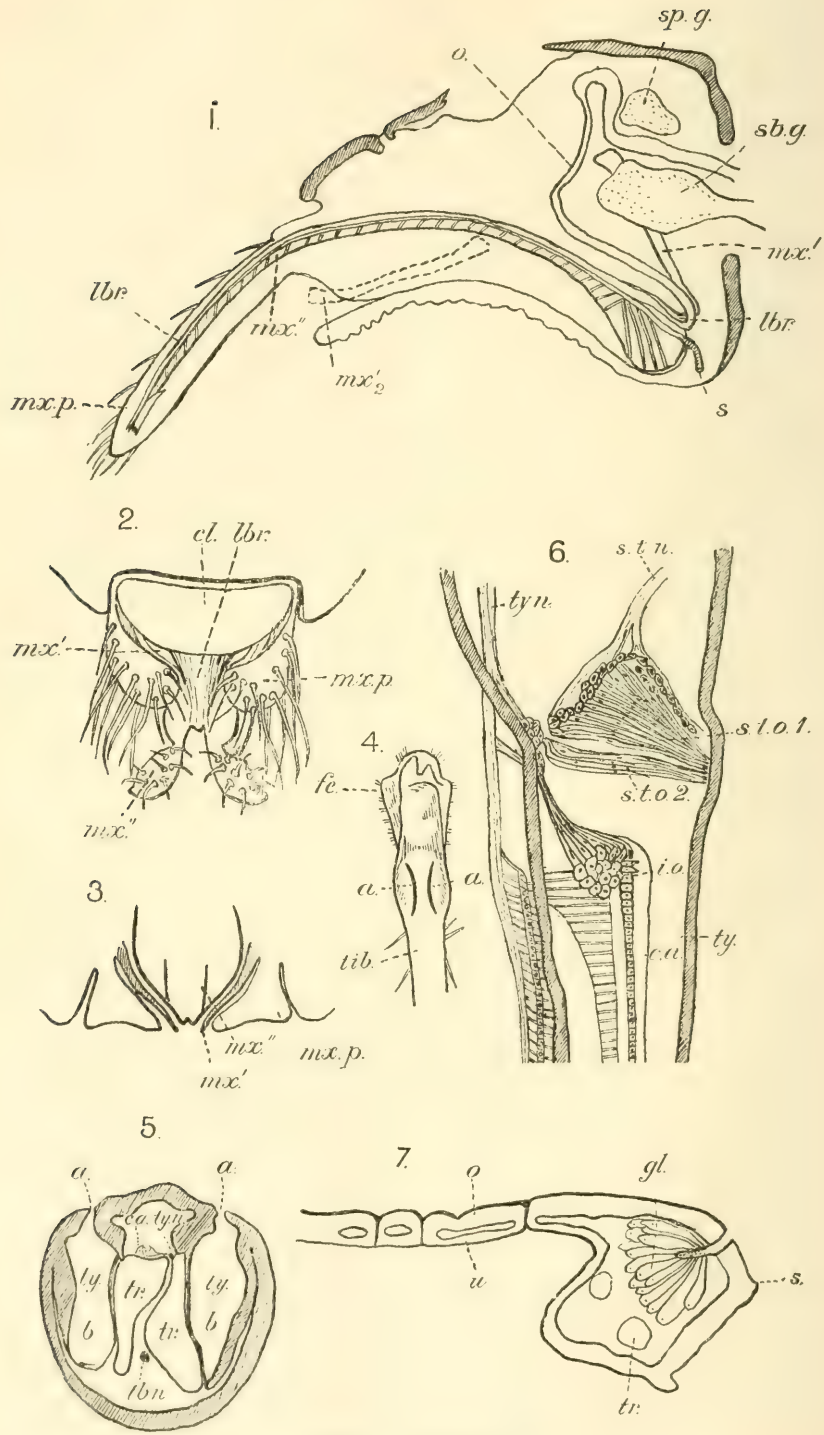
[1] In the last number of the *Ann. Mag. Nat. Hist.* (6), vol. xi., p. 45, Mr. C. O. Waterhouse also criticises Professor Lowne's views on the structure of the proboscis in the Diptera.

the Hippoboscidæ and in *Braula* and *Nycteribia*, elongated in the former and short in the latter group. Müggenburg supports the generally received view that the true proboscis is formed by the labrum, hypopharynx, and second pair of maxillæ (labium). In the Hippoboscidæ this organ is long and flexible; in *Braula* and *Nycteribia* it is much shortened, and the formation of its ventral (labial) part by the fusion of a pair of jaws seems very clear (Fig. 2, mx^{II}). In the latter insects, also, the maxillæ and their palps are connected at their bases; the mouth-parts have altogether a more typical aspect than in Diptera generally; and it would seem that, in the course of the degradation consequent on their parasitic habit, the jaws of these insects have reverted in some degree towards a primitive form.

Our knowledge of the ears of insects has lately received another contribution. Organs of hearing have not been recognised in many groups, but they have long been known in the jumping Orthoptera, the insects included under the popular terms grasshoppers and locusts. In the short-horned family of these insects, generally known as the Acrididæ, but henceforth in accordance with the law of priority to be styled Locustidæ,[2] the organs of hearing are paired, and situated on either side of the first abdominal segment. In the long-horned group, which is called Locustidæ by most entomologists, but which we must now learn to know as Phasgonuridæ, the ears are placed in the upper part of the tibial joints of the front pair of legs. The tibia is swollen just below the knee, and two slightly curved slits can be seen running longitudinally along the joint for a short distance (Figs. 4, 5). These are the openings into the outer auditory chambers. An account of these remarkable ears has lately been published by Von Adelung (3) who has studied their structure in the genera *Phasgonura* (*Locusta*, auct.), *Decticus*, *Thamnotrizon*, and *Meconema*.

The slits mentioned above open into chambers formed by the inpushing of the integument of the leg. These chambers (Fig. 5) are thick-walled outwardly, but their inner walls are thin, and form the two oval tympana or drums, each of which is in contact with the wall of a trachea, or breathing-tube, the tracheal stem dividing in this region into two parallel branches, whose walls, however, remain in contact. The interior of these air-vessels functions as an internal auditory chamber. Along the outer wall of one of the air-vessels runs a ridge of tissue—the crista acustica—in which are arranged in linear series the cone-shaped endings of the nerve-fibres, each capped by a large cover-cell. These nerve-endings and their cover-cells become successively smaller from top to bottom of the series, which stretches parallel to and about as long as the longer axis of the drum.

[2] This change is less objectionable than many recent revolutions in nomenclature, as the true locusts, which belong to this family, will henceforth be called Locustidæ. Under the former arrangement they were excluded from the family named after them.



INSECT ANATOMY.

From each cone the nerve-fibre passes along the wall of the tracheal tube, first to a ganglion cell, and then to join the tympanal nerve, which is situated where the tracheal wall comes into contact with the drum (Figs. 5, 6).

Near the upper end of the crista acustica is situated a mass of nerve-endings which have hitherto been reckoned a part of that structure, but for which Von Adelung proposes the name of "intermediate organ" (zwischenorgan) (Fig. 6, i.o.). These nerve-endings, which are connected by nerve-fibres with ganglion-cells, and then with the tympanal nerve, are also in connection with tissue-fibres, by which they are stretched between portions of the integument of the leg. Hence, Von Adelung considers them related to the rods of the chordotonal organs described by Gruber (4) as well as to the nerve-endings of the supra-tympanal organ which forms the remaining portion of the ear of the long-horned grasshoppers.

This supra-tympanal organ consists of two groups of elongated rods stretched across within the tibia above the region of the drums. These rods are connected with ganglion cells. From the ganglion-cells of the lower group fibres pass to the tympanal nerve, while those of the upper group are connected with a special supra-tympanal nerve, which, like the tympanal, takes its origin from the main tibial nerve (Fig. 6, s.t.o.).

An interesting paper on the structure of insect-wings has recently appeared. Hoffbauer (5) nas investigated these organs by means of

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EXPLANATION OF FIGURES.

Fig. 1.—Longitudinal section through head of *Melophagus* (proboscis retracted).
2,—Mouth-parts of *Braula* (front view).
3.— Do. (horizontal section.) [All after Müggenburg.]

cl.—clypens.
lbr.—labrum.
mx.^{1}—maxillæ (first pair).
mx.$^{1}{}_{2}$—Do., when proboscis is exserted.
mx. p.—Do., palp.
mx.^{11}—maxillæ (second pair) (or labium).
o.—œsophagus.
s.—duct of salivary gland.
sp. g.—super-œsophageal ganglion.
sb. g.—sub-œsophageal ganglion.

Fig. 4.—Sketch of tibia of a Phasgonurid, to show position of ear apertures.
5.—Transverse section of tibia.
6.—Longitudinal section, showing parts of ear. [All after von Adelung.]

fe.—femur.
tib.—tibia.
a.—external apertures.
b.—outer auditory chambers.
ty.—tympana.
c. a.—crista acustica.
tb. n.—tibial nerve.
ty. n.—tympanal nerve.
s.t.n.—supra-tympanal nerve.
tr.—tracheæ.
i. o.—intermediate organ.
s.t.o.$_{1}$—supra-tympanal organ (upper division).
s.t.o.$_{2}$—supra-tympanal organ (lower division).

Fig. 7.—Transverse section through sutural edge of elytron of a Chrysomelid beetle (*Lima pulli*). [After Hoffbauer.]

o.—upper surface.
u.—under do.
s.—sutural edge.
tr.—trachea.
gl.—glands.

transverse and longitudinal sections. It has long been known that the wings of insects are formed by the outgrowth of a fold in the thoracic integument, and that they therefore consist of a double membrane. Sections show the membranes lying apposed at most parts of the wing-area; but when the section passes through a nervure the membranes are separated to enclose a tubular space, in which are an air-vessel, blood-passages, and fat body.

It is not, however, to membranous wings, such as those of flies, bees, dragon-flies, or the hind wings of beetles that Hoffbauer has mainly directed his researches, but to the horny front wings of the latter insects, the wing-cases or elytra as they are generally called. Here the lamellæ become thick and chitinous; the upper lamella is sharply folded inwards in places, forming striæ above and making transverse supports between the two surfaces of the elytron. Along the sutural border of the elytron, the lamella forms a tubular space within which are numerous glands which occur in groups leading into common ducts, which open in several series along the suture (Fig. 7). In some cases the glands occur beneath the disc of the elytra, over the surface of which the ducts then open.

To most readers, the question of the homologies of the elytra of beetles as discussed by Hoffbauer will be of interest. They are almost universally accepted as corresponding to the front pair of wings in other insects. Meinert and others have, however, suggested their homology with the tegulæ of Hymenoptera, small scale-like processes in front of the fore-wings, and have imagined the atrophy of the true fore-wings in beetles. Hoffbauer notes a correspondence in structure between the elytra and the sides of the pronotum, and suggests their origin as lateral outgrowths of a thoracic tergum of which the scutellum represents the central part. But this does not contradict the generally-received view that they are modified fore-wings, since the origin of all insect-wings as outgrowths of the thoracic terga is admitted. The presence of two pairs of wings, always connected with the two hinder thoracic segments, throughout all groups of living and extinct insects, is certainly strong evidence in favour of their homology throughout the class; and in the various groups of the Rhynchota we can find a series which goes far to bridge the structural gap between the wing of a fly and the elytron of a beetle.

The wings of insects are studied from another point of view by Spuler, who has given (6) an account of their neuration in various groups of Lepidoptera. He believes that in the most primitive type of neuration the fore- and hind-wings have a similar arrangement of nervures. This occurs in the Neuroptera, and, among the Lepidoptera examined by him, in the genera *Hepialus* and *Micropteryx*, which must therefore be regarded as primitive. The tendency in most groups of the Lepidoptera is towards a reduction in the nervures of the hind-wings. In the development of the individual this rule seems to be followed; the nervures of the hind-wing in the pupa are more

numerous than in the butterfly or moth. Also an arrangement of parallel or forked nervures in the pupa becomes in the imago a neuration with transverse branches forming "cells." Spuler divides the insect-wing into two regions, a smaller "folding part," comprising two or three nervures near the inner margin, and a larger "spreading-part," comprising the rest of the wing-area.

In a later paper (7) he applies these results to a special group, the Papilionidæ. The genus *Thais* is believed to represent the ancestral form of the family whence the *Parnassii* have diverged in one direction and the various species of *Papilio* in the other. A "genealogical tree" is given, showing the supposed relationships of these to each other and to their ancestral stock. Those who have experience of such "trees," will not be surprised that the lines of descent often differ from those indicated by Eimer in his well-known work (8). Nevertheless, the object of natural history research is to discover the natural relationship between living creatures, and such speculations, though they can never be final, are full of interest. The arrangement of the nervures on a butterfly's wing becomes a subject of real importance in the light of the doctrine of descent.

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GEORGE H. CARPENTER.

V.

Parasites on Algæ.

LIKE most other subjects that take their turn in popularity as fields of research, this one is by no means so new as may readily be thought. There are few things in Nature that our "rude forefathers," with their ruder appliances, did not grapple with after some fashion. The earliest mention of any fact that may be classed under this heading is probably the discovery of the so-called "galls" in the well-known siphoneous Alga *Vaucheria*. They were described and figured by Vaucher (1) so long ago as 1803, and many observers since then have added to the list of species infested, and have described the parasites and their operations. Of these writers, Balbiani (2) has given the most exhaustive account. The parasite in this case is an animal, and is described in the earlier papers as *Cyclops lupula*, but Balbiani and others refer it more accurately to *Notommata werneckii*. In a paper on the subject by Professor Oliver (3), the animal is given on the authority of the late Mr. Gosse as probably *Rotifer vulgaris*. The parasite enters a lateral fertile branch at an early stage, and sets up hypertrophy, causing it to swell to four or five times its original size. These galls have been frequently observed, as has been said, and an extensive literature has arisen on the subject, which will be found fully cited by Mr. A. W. Bennett (4), who gives from Benkö the following list of species on which they have been observed:—*V. racemosa*, *dichotoma*, *clavata*, *caespitosa*, *geminata*, *uncinata*, and *terrestris*. Lister observed them on *V. aversa* and *V. dillwyni*.

It is extremely probable that a considerable number of animals make use of Algæ as their hosts, as they do of land plants; but botanical literature, so far as I can discover, contains remarkably little about it. Miss Barton (5) has described malformations of the thallus of the common dulse, *Rhodymenia palmata*, caused by a copepod, *Harpacticus chelifer*, which inhabits the tissues and burrows in them during a stage of its existence. The same enthusiastic phycologist (6) has recently described two other similar cases, viz., gall-like structures on *Desmarestia aculeata*, also caused by a copepod (too immature for determination), and very remarkable malformations of *Ascophyllum nodosum*, caused by a nematode worm, *Tylenchus fucicola*. This worm, nearly related to the well-known "wheat-eels," has been minutely described and beautifully figured by Dr. de Man (7).

With the discovery by Alexander Braun (8) of a Chytridian parasite on the fresh-water Alga *Hydrodictyon*, and the publication, in 1855, in the paper quoted, of some twenty species of *Chytridium*, many of them inhabiting fresh-water Algæ, a new field of research was opened to cryptogamists. In the same year Bail (9) extended our knowledge of the subject, and Cienkowski (10), a little later, described a *Rhizidium* parasitic on *Conferva glomerata*. Cohn (11), in a remarkable paper on the physiology of the Florideæ, pointed out, in 1867, that *Chytridia* had been mistaken for fruits in certain marine Algæ, and thus extended the domain of this group to the sea. He was soon followed by Magnus (12), who made known other forms, of which the following may be looked for by students of our native Algæ, viz., *Chytridium tumefaciens*, in species of *Ceramium*; *C. sphacelarum*, in *Cladostephus spongiosus*; and *Sphacelaria cirrhosa* and *C. plumulæ*, in *Callithamnion*. Kny (13) next described another form—*C. olla*—in the fresh-water *Œdogonium rivulare*, and was succeeded by Nowakowski (14) and Professor Perceval Wright (15) with further records. In the meantime Pfitzer (16) described an interesting, novel parasite on *Closterium*, viz., *Ancylistes closterii*, which is of such singular character that it is commonly reckoned by itself as the type of a group, the Ancylisteæ, unless one puts with it *Lagenidium*, of which Zopf (17) described a form inhabiting *Spirogyra*, though there is now a disposition to include these forms under Chytridiaceæ. These Chytridiaceæ are a group of Fungi of aberrant type, and it is still debatable whether they form one natural family, or are better considered a convenient assemblage of forms with affinities in various directions among Fungi, or even with Protococcaceæ among Algæ. Both views claim a good deal of support, but much remains to be done in working out the life-histories. They are parasites in the tissues of marsh and aquatic plants, including aquatic Fungi such as *Saprolegnia*, and are reproduced by the production of swarm-spores in sporangial cells of definite shapes. These swarm-spores are provided, as a rule, with only one cilium, and they develop without well-marked intermediate stages into fresh sporanges. Some forms have resting spores which develop in similar fashion. They are all exceedingly minute, and easily find lodgment in the cells of their hosts, on which they produce both destructive and deforming effects. An excellent general account of their life-histories and operations will be found in De Bary's *Vergleichende Morphologie und Biologie der Pilze*, of which the Clarendon Press has published an English translation by Garnsey and Balfour, while the latest systematic accounts of them (both well illustrated) are by Alfred Fischer in *Rabenhorst's Kryptogamen-Flora*, pt. i., vol. iv., lieferungen 45 and 46 (1892), and by Schrœter in Engler and Prantl's *Naturl. Pflanzenfam.*, lieferung 76, p. 64. Not the least interesting circumstance about them is their occurrence in the sea, since it is well known that even such aquatic Fungi as the *Saprolegnia* of the salmon-disease find immersion in salt-water rapidly

fatal. A few other Fungi, such as certain parasites on *Zostera*, a marine flowering plant, have been recorded with some doubt so far as their actual growth in sea-water is concerned.

This parasitism of Fungus on Alga naturally suggests the case of the lichens, where, however, both classes of organisms dwell together in amity and their relations are symbiotic rather than parasitic.

Professor Perceval Wright (18) described in 1879, in an admirable paper, "a new species of parasitic green Algæ belonging to the genus *Chlorochytrium* of Cohn," which he found inhabiting the fronds of *Schizonema*, *Polysiphonia*, &c. A consideration of this form opens fresh ground, since it, like the type form of *Chlorochytrium*, inhabiting duck weed, described by Cohn (19) in an interesting paper on parasitic Algæ, does not appear to be a case of true parasitism, but rather of that sort called by Klebs "Raumparasitismus," in which the host furnishes merely lodging without board to the intruder. There are many such cases among the lower Algæ, and their bearing on symbiosis and on the parasitic habit itself is an instructive one. A large number of Algæ (especially marine forms) live as epiphytes on larger Algæ, and, indeed, constantly select the same species of host and the same part of its thallus. Many of these deserve special investigation, since cases occur in which the rhizoids of the epiphyte (and even wedges of its tissue, like haustoria) penetrate the host, and if not actual burglars are certainly in a very compromising position. We have an example of more than mere "Raumparasitismus" (we thank Klebs for that word) in *Phyllosiphon arisari*, a siphoneous Alga which lives in the intercellular spaces of the leaf of *Arisarum vulgare* and even consumes the chlorophyll in the adjacent cells. Other Algæ are known to inhabit the tissues of flowering plants, of *Azolla* and of Muscineæ, but the consideration of these is at present leading us away from the subject. *Chlorochytrium* (which may be put among the Protococcaceæ) furnishes us at any rate with a case of symbiosis, so far as shelter is concerned, between Algæ, and the line of investigation so successfully pursued by Professor Perceval Wright is one that phycologists may be confidently incited to follow.

Finally, and of special interest, are certain tubercles on the fronds of Florideæ described last year by Dr. Schmitz (20) as caused by Bacteria. The numerous cases of error in attributing the causes of diseases to Bacteria may make one unduly cautious in accepting statements of this kind when the possibility of the Bacteria being merely *post hoc* has not been absolutely excluded by the evidence of inoculation experiments. Dr. Schmitz, however, is not the man to make rash statements, and his research is a very noteworthy one, and one, moreover, that suggests other matters of interest in regard to Bacteria.

I am convinced that the subject of the parasites of Algæ—as well as that of parasitic Algæ—is but in its infancy, and in the hope that workers may be attracted to a field of research of great difficulty,

but with rich promise of reward, I have appended a list of papers which, while it does not claim to exhaust the literature by itself, will yet, with the books referred to in the text, guide the student to all that is known of the subject, and to collateral matters of interest.

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GEORGE MURRAY.

VI.

The Underground Waste of the Land.

GEOGRAPHERS and geologists are alike interested in the origin of the physical features of the land; but their sympathies become united only when we treat of the actual processes of sculpture. To the geologist the evolution of scenery is an exceedingly complex subject, for many of the features marked out in ancient epochs have been buried up by subsequent sediments and afterwards revealed by denudation. In the long and varied history of the subject the geographer is apt to manifest impatience, for he cares little about the age of the rocks, so long as he understands their relation to the form of the ground, and the influences that have contributed to produce the present shape of hill and dale, lake and river.

In this country the waste along our coasts, especially along the eastern and southern shores, is manifest; but when we study the inland features and find evidence of so many ancient earthworks, it would seem that the surface of the land has been but little modified during the past four or five thousand years.

The power of rain and rivers in wearing away the surface of the land is scarcely realised until statistics are presented to us of the amount of solid matter annually carried to sea by our rivers. From some areas in England and Wales as much as 150 (or even 200) tons per square mile may be removed each year, but the result is imperceptible, for it means a lowering of the general level of the land of about one-tenth of an inch in a century.

More conspicuous are the local evidences of destruction that may be seen in the occasional landslip and in the screes of fragmentary rock at the foot of crag and mountain. The material is dislodged by the mechanical action of frost and rain, and in some situations, as along the Pass of Brander, below Loch Awe, it is carried away by torrential streams, and the rounded fragments go to form beds of gravel. Again, after heavy rains, the turbid streams in the lowlands plainly indicate the kind of denudation that is taking place.

The loss of material that is carried away in solution is rarely made manifest, except in the case of caverns, and by sinkings of the ground in limestone-areas; or by the artificial removal of brine from the salt regions of Cheshire, whereby great subsidences have been caused. Even the Bath hot-springs, which do not contain a very

large amount of mineral ingredients compared with some other saline waters, daily carry away between three and four tons of solid matter, and the removal of this must cause cavities. Perhaps the landslips that have occurred from time to time at Bath may be to some extent influenced by subterranean movements, caused by the filling-in of cavities by the overlying strata. The waters of Burton-on-Trent may be cited in reference to chemical erosion, and it has been estimated that about 150 tons of gypsum are annually imbibed in potations of Burton beer.

In the more hilly and mountainous regions, where bare rocks frequently jut out at the surface, the evidences of destruction are apparent. Among the gentler hills and vales of the midland and southern counties, the evidences of erosion are to be judged mainly by the solid matter carried away by the streams. The matter held in solution is due chiefly to subterranean erosion; that held in suspension is due almost entirely to superficial erosion. The hills are, for the most part, composed of limestones, sands, and sandstones, through which the rain-waters may percolate, until arrested by a band of clay, when they issue as springs. Hence the hills are not so much subject to superficial denudation as are the vales, for these lie mainly in tracts of clay that are directly acted upon by the streams that flow across them. The main features of hills have, therefore, remained permanent for long ages, though their general level may have been continuously, if imperceptibly, lowered.

The question arises whether the underground erosion may not be partly mechanical, even if it be mainly chemical. Underground waters that flow on the top of a mass of clay must form channels in that material. The permanence of springs that issue here and there along the foot of escarpments, indicates plainly that the underground waters follow definite courses; and the actual outlet of such springs may be found at varying levels beneath the plane of division that separates the porous and impervious strata, because the springs have eroded their channels in places, perhaps, to depths of five or six feet.

Referring to the Central Himalayan region, Mr. C. L. Griesbach (4) mentions a limestone which rests conformably on calcareous shales, and this, like most limestones, is much jointed; consequently, all the drainage finds its way through the joints into the underlying shales. These become disintegrated, and are gradually carried away, while the thick limestone band sinks down to the level which was formerly occupied by the shales.

In an article in which I discussed the origin of the Scenery of Norfolk, I remarked that on the clayey strata the rainfall must accumulate or flow away at once towards lower levels; on the sandy and gravelly strata it will sink down until arrested by impervious beds beneath. Hence the earliest exposed channels no doubt commenced on the clayey areas that formed the surface. Beneath the

gravelly and sandy tracts, the rainfall formed subterranean courses, flowing, in some instances, even out to sea in that way, as we now witness in the case of numerous springs issuing from our cliffs on the coast. There deep channels or "chines" are in time formed, and the gravelly accumulations being ultimately cleared out, the clayey strata are exposed. In one case, I noticed a narrow gorge in the Contorted Drift, 9 feet deep and 2½ to 3 feet wide—like a miniature cañon—at the bottom of one of these chines. Thus streams flowing for a time underground, and carrying away material from below, will cause the surface to sink.

From these and other considerations we may be able to understand how some of our great sheets of gravel and sand, like those of the neighbourhood of Holt and Cromer, have become isolated hills, with ramifying spurs; for although some of the larger sheets of gravel may have been deposited in patches, yet the surrounding strata have been denuded, and they themselves have been broken up into smaller patches or outliers. When once this has taken place, the surface-features of these sandy and gravelly tracts may remain for long periods much the same, although the level of the whole may be slowly reduced by springs carrying away material from the lower portions of the strata at their junction with clayey beds beneath (3).

Subterranean erosion is suggested by the irregular channels that are often met with on the surfaces of clays that lie beneath Valley Gravel. On the Lias Clay at Bath, on the Oxford Clay at Oxford, and on the London Clay in the Thames Valley, we find such channels beneath the gravel; and in many instances the stones filling the channels lie with their longer axes more or less upright. These appearances have sometimes been attributed to the action of land-ice, or to the movements of thawing and slipping soil.[1] Without questioning that these explanations may be true in certain cases, I think the possibility of erosion by streams has often been overlooked.

On the Dorsetshire coast, between Lyme Regis and Bridport, gullies similar to those noted on the Norfolk coast may be seen in the Lias clays, and these are formed by springs that issue from the porous Cretaceous Beds above. Conybeare, in 1840, in his explanation of the great Landslip of Dowlands and Bindon, remarked that where the loose sands of the Upper Greensand rest on the Lias clays, and are exposed in the cliffs, "copious land springs will gush forth, and carry away in different seasons greater or less quantities of the loose material through which they flow; and thus, in process of time, the superincumbent rock will become partially undermined."

Some of the landslips on Portland, and the great gullies or fissures that affect the limestone-rocks, may probably be attributed

[1] *See* Letter of Darwin to Professor James Geikie (1876), *Life and Letters of Darwin*, ed. 2, 1887, vol. iii., p. 214.

to this undermining action of land-springs, and the consequent disruption of the strata.

Thus the level of the land may be lowered by springs carrying away sand, and also by their eroding channels in the clays over which they take an underground course. We may also discern a way in which outliers have been produced, many of which, like Glastonbury Tor and Brent Knoll in Somersetshire, appear to owe their preservation to a gentle, basin-shaped structure in the arrangement of their strata. Mr. T. V. Holmes has pointed out that some of the outliers of Bagshot Beds in Essex probably owe their existence to similar causes.

Minor instances of erosion beneath clay may sometimes be noticed. Thus in seasons of drought a superficial layer of clay may contain deep cracks, and bands of limestone three or four feet from the surface may bear evidence of erosion by carbonated water. Joints in limestone that have been widened into fissures by chemical erosion are often filled up with clayey material from beds above, and when the rocks are much fissured and eroded, small dislocations sometimes occur that produce a kind of faulting which does not affect the underlying strata. In these ways the level of the land is slightly lowered in many places.

Lyell (1) has referred to the depression of "submarine forests" by the drainage of peaty soils on the removal of a seaward barrier that formerly prevented the escape of the waters. His attention was drawn to the subject by observations published by the Rev. Dr. Fleming; and Lyell applied the explanation to a case at Bournemouth. There "the sea, in its progressive encroachments, eventually laid bare, at low water, the foundations of this marshy ground; in this case, much of the sand, of which these foundations were composed, might have been washed out by the rapid descent of the fresh water through them, at the fall of the tide."

John Cunningham (2), as pointed out by Mr. G. H. Morton (6), gave a somewhat similar explanation of the submarine forest of Leasowe; and very recently Mr. William Shone (5) has drawn attention to the subject in a paper referring to the same district. It must be confessed that these suggestions have yet to be substantiated.

Nevertheless, the subject of subterranean erosion by mechanical agency is one to which more attention may profitably be directed. The observations here recorded tend to show that the amount of erosion, whether mechanical or chemical, great as it must be, is hardly perceptible except locally. Our main features are of great antiquity, and, although the present forms of escarpments are due to subaërial agents, yet but a small amount of rock (a mere "notch") has been removed in this way compared with original extent of the strata. Into that particular subject it would, however, be undesirable now to enter, my object being simply to draw further attention to the question of underground erosion; and to point out how it is

that the general level of the land may be lowered by the material removed in solution and in suspension by springs, streams, and rivers, while the surface-features of hills and plateaux exhibit so little change.

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Horace B. Woodward.

VII.

Owen.

(*Concluded from page* 30.)

IV.—SIR RICHARD OWEN'S RESEARCHES ON THE VERTEBRATA.

SIR RICHARD OWEN'S contributions to knowledge of the vertebrate animals, both living and extinct, are so vast, and relate to so many details of anatomy and zoology, that it is impossible in a brief notice to do more than survey the broad features of his work. He was placed, it is true, amid facilities for research that have perhaps not been equalled either before or since; he not only benefited by the labours of John Hunter, and had the entire collection of the Royal College of Surgeons at his disposal, but for a long period was favoured with the almost exclusive right of dissecting the various animals dying in the Zoological Gardens. Nevertheless, none but an enthusiast gifted with Owen's great intellect and indomitable perseverance could have taken the full advantage of these facilities; and the mass of new facts and detailed observations in Comparative Anatomy and Zoology recorded in the Hunterian Professor's publications, exceeds even the work of Cuvier himself. His various catalogues of the collections in the Royal College of Surgeons, his three volumes on the *Comparative Anatomy and Physiology of Vertebrates*, and his numerous memoirs published by the Royal, Linnean, and Zoological Societies of London—all must be tested to be appreciated; and we fear there are few naturalists of the newest school who refer to these old classics to the extent that they deserve.

Owen's detailed memoirs and descriptions, however, require laborious attention in reading on account of their nomenclature and modes of expression; and there is, perhaps, some reason that those now engaged in research should confine their attention to works of reference in more familiar language. At the same time, it must be remembered that Owen was the pioneer in a concise anatomical nomenclature; that many of our most familiar terms of everyday use are due to him; and that if the "laws of priority" are ever enforced as regards anatomical terms, his influence on accepted nomenclature will become still more conspicuous. It is, indeed, to be regretted that Owen could never be induced to follow, at least to some extent, the new school of anatomy and zoology that arose with the epoch-

making researches of Von Baer and Rathke in embryology; and it is precisely that unwillingness to depart from the philosophical ideas of his earlier life that has led to Owen's temporary eclipse by the present generation.

This marked disregard of embryology as the essential adjunct, even if not the key, of comparative anatomy, is all the more surprising, since so large a proportion of Owen's researches on vertebrate animals were devoted to the fossil remains of past ages. If any phase of biological research can benefit by embryology, that is assuredly palæontology; and it is strange to have to record that not only did Owen fail to appreciate this fact, but that he absolutely ignored some of the most striking memoirs in which an attempt is made to utilise modern methods, and discover the successive stages through which any particular type of animal has passed during geological time. His statements on the succession of genera and species, and their possible derivation one from another, were always vague, and capable of more than one interpretation; and though there is not much doubt he leaned towards the views of Geoffroy St. Hiliare, and those who believed in the evolution of life, his work, for the most part, is eminently Cuvierian—a laborious description of the facts, with a detailed discussion that rarely extends beyond strict comparative anatomy and the phenomena of geographical or geological distribution. Only on two occasions[1] does he appear to have attempted any broad philosophical deductions, and, even in those cases, it is not quite clear how much he admits. He was perfectly well aware that the facts of progression noticed by the anti-evolutionist Agassiz among fishes were equally conspicuous among the higher vertebrates[2]; but he contented himself with the bare statement that "the inductive demonstration of the nature and mode of operation" of the laws governing life would "henceforth be the great aim of the philosophical naturalist."

Owen, in fact, was Cuvier's direct successor, and apart from his striking hypotheses to which Dr. Mivart has referred,[3] it is in this character that he has left the deepest impression upon biological science. Extending and elaborating comparative anatomy as understood by Cuvier, Owen concentrated his efforts on utilising the results for the interpretation of the fossil remains—even isolated bones and teeth—of extinct animals. He never hesitated to deal with the most fragmentary evidence,[4] having complete faith in the principles established by Cuvier; and it is particularly interesting in the light of present knowledge to study the long series of successes and failures that characterise his work. However unwittingly, Owen may

[1] References to horse in "Anat. and Physiol. Vert.," vol. iii., p. 791 (1868), and to crocodiles in *Quart. Journ. Geol. Soc.*, vol. xl., p. 157 (1884).

[2] "Palæontology," ed. 2, p. 444 (1861).

[3] NATURAL SCIENCE, vol. ii., p. 18.

[4] Cf. *Anthrakerpeton crassosteum*, Owen, *Geol. Mag.*, vol. ii., p. 6, pls. i., ii. (1865).

be said to have contributed most to the demolition of the narrow Cuvierian views; when dealing with animals closely related to those now living, his correctness of interpretation was usually assured—when treating of more remote types he could do little more than guess, unless tolerably complete skeletons happened to be at his disposal. The fragmentary thigh-bone of *Dinornis*, brought to him in 1839, sufficed for the great anatomist to demonstrate that gigantic flightless birds had once existed in New Zealand; and the single fragment of the end of the lower jaw of *Diprotodon* from New South Wales was enough to enable him to conceive the former existence of that huge wombat-like animal in Australia. When, however, the molar teeth and femur of *Diprotodon* were first discovered, Owen failed to recognise their relationships and described them as evidence of a "Dinotherioid or Mastodontoid pachyderm" in Australia; and the restoration of the "great horned lizard" of Queensland (*Megalania*) has proved even more unfortunate, the backbone only belonging to a lizard, the head and tail to a tortoise, and the toes to a marsupial quadruped. The supposed tooth of a monkey from the Eocene of Woodbridge soon proved to belong to a primitive hoofed animal; and there is no longer any doubt that the comparison of *Stereognathus*, from the Oolites, with the Ungulata is based upon a complete misapprehension. In short, Owen's work on fragmentary fossils has demonstrated that the principles of comparative anatomy are very different from those inferred by Cuvier from his limited field of observation; and the discoveries of Leidy, Marsh, Cope, Scott, and Osborn in America have finally led to a new era that Owen only began to foresee clearly in his later days.

Throughout this work, one of the most striking features was the persistence with which Owen followed each subject until he had exhausted all available material. Impressed, in the earlier part of his career, with the researches of Retzius and others on the minute structure of teeth, he discussed and extended these observations until he had studied in detail the teeth of every known division of the vertebrates, living and extinct; and thus was produced his unrivalled work on *Odontography* (1840-45). He began his researches on British fossil mammals, birds, and reptiles, also, by contributing exhaustive summaries of all known material to the Reports of the British Association; and, although his results were usually published in small instalments in scientific serials, he nearly always arranged that the various groups of papers should follow in connected sequence, and in many cases he had them reprinted in the form of successive sheets of separate works, which appeared as *Researches on the Fossil Remains of the Extinct Mammals of Australia, with a notice of the Extinct Marsupials of England* (2 vols., 1877), *Memoirs on the Extinct Wingless Birds of New Zealand* (2 vols., 1879), and *A History of British Fossil Reptiles* (1849-84). This method is convenient for purposes of reference, but it naturally led to continual disagreement between the author of the memoirs and

the councils of the societies from whose publications they were extracted.

To refer now more particularly to some of Owen's principal contributions to knowledge of the vertebrata, we have first to allude to the fishes, which did not attract so much notice as the higher groups. Besides his detailed descriptions in the Hunterian lectures and catalogues, however, there are some of Owen's writings that will ever remain memorable. His description of the African mud-fish *Protopterus*, for example, laid the foundations for the recognition of the Dipnoi by Müller, and it is interesting to note that Owen emphasised the resemblance between the teeth of this fish and the fossils named *Ceratodus* long before anything was known of the affinities of the latter. He perceived the direct serial connection between the teleostean and ganoid fishes, and grouped them in one sub-class, the Teleostomi. Among fossils, too, he made several advances, and to him we owe the first information of the complex structure of the teeth of the Holoptychian fishes named *Dendrodus*—a structure, as Owen pointed out, only paralleled in the terrestrial extinct Labyrinthodonts.

The discovery of the remarkably complex structure of the last-mentioned teeth, which Owen referred to a group of animals termed Labyrinthodonts, was one of his earliest contributions to knowledge of the extinct cold-blooded air-breathers. On discussing some portions of the skeleton later, he concluded that these animals were probably amphibia, and it is only quite lately that much evidence has been brought forward to indicate their higher rank. At the same time, Owen can scarcely be held responsible for the great frog-like "restoration of *Labyrinthodon*" that is commonly ascribed to him; he merely interpreted the fragmentary bones as best he could, and the monstrosity just referred to was the unjustifiable work of a "populariser" of scientific investigation.

Among undoubted reptiles, Owen's first important triumph was his recognition of the great group of Mesozoic land-reptiles, to which he gave the now-familiar name of Dinosauria. Hermann von Meyer, it is true, had already expressed some vague ideas on the subject of what he afterwards termed the "Pachypoda"; but Owen, with the assistance of Mantell and Buckland, was the first to arrive at so much precision as could be attained in those days, and he continued to make the principal contributions to our knowledge of Dinosaurs for a period of nearly 40 years, describing *Iguanodon*, *Hylæosaurus*, *Cetiosaurus*, *Omosaurus*, *Scelidosaurus*, *Megalosaurus*, and more or less fragmentary remains of other genera. Even to the end, however, Owen failed to appreciate some of the fundamental characters of these animals,[5] and there is now not much doubt (reasoning from

[5] *Cf.* Restoration of skull of *Megalosaurus* in *Quart. Journ. Geol. Soc.*, vol. xxxix., p 340 (1883).

discoveries of complete skeletons in America) that they belong to more than one order of the reptilian class.

The Anomodontia—those curious early Mesozoic reptiles combining in their skeleton certain features now only known in amphibia and mammalia—were also first recognised and defined by Owen, who described a great number of forms from the Karoo deposits of Cape Colony, beginning with *Dicynodon* in 1845, culminating in his exhaustive *Catalogue of the Fossil Reptilia of South Africa* (British Museum, 1876), and even further pursued in later papers. The discovery of *Rhynchosaurus* in the English Keuper was also the first evidence of the order recognised by later observers as the Rhynchocephalia. Moreover, the most elaborate series of descriptions of the Ichthyosauria and Plesiosauria are due to Owen; and he has made known nearly all the principal British types of fossil Chelonia and Ophidia hitherto described. His memoir on *Dimorphodon* marked a great advance in knowledge of the Pterosaurian skeleton; and his discovery of the reptilian nature of *Placodus* and *Stagonolepis* (previously regarded as fishes) was also an important step.

Among contributions to the anatomy of birds, we may specially refer to Owen's classical memoir on the apteryx, and his further notices of the anatomical characters of the flamingo, pelican, gannet, and hornbill, all published by the Zoological Society. His important series of memoirs on the osteology of extinct birds, however, issued by the same society, excel even his work on living forms, and we need only recall his descriptions of the dodo and great auk, besides his numerous memoirs on the Dinornithidæ, on *Aptornis*, and on *Notornis* from New Zealand. From the palæontological standpoint, too, Owen's monograph on *Archæopteryx*—the long-tailed, toothed bird from the Bavarian Lithographic Stone—is an epoch-making work. Finally, he described *Odontopteryx*, *Argillornis*, and *Dasornis* from the London Clay.

Among recent mammalia, the more striking of Owen's contributions relate to the monotremes and marsupials, and the recognition of the two natural groups of typical Ungulata—the odd-toed (Perissodactyla) and the even-toed (Artiodactyla); and mention ought also to be made of his studies of the various apes. His attention seems to have been first directed to the extinct mammalia when he undertook the description of the fossils collected by Darwin in South America during the voyage of the "Beagle." *Toxodon* was then described, and thus provided the first clear evidence of an extinct generalised hoofed animal; according to Owen, indeed, this was a "pachyderm, with affinities to the Rodentia, Edentata, and Herbivorous Cetacea." *Macrauchenia*, erroneously associated with the camels, was also made known, and there were descriptions of new gigantic sloths, *Mylodon* and *Scelidotherium*. Almost at the same time, too, Owen proved that *Megatherium* was not an armoured animal, but that the dermal plates often ascribed to it really per-

tained to a non-banded armadillo, to which he gave the name of *Glyptodon*.

While dealing with the South American extinct mammals, Owen was also occupied with bones and teeth from the caverns of New South Wales, discovered by Sir Thomas Mitchell, and thus he laid the foundation of the long series of memoirs on the extinct mammals of Australia, for which he obtained materials from Dr. George Bennett and many other indefatigable correspondents. Moreover, in the midst of this activity, he was collecting materials for an exhaustive work on the British fossil mammals, and his charming volume on *British Fossil Mammals and Birds* appeared in 1846.

Owen also took part in the discussion on the supposed mammalian jaws discovered in the Stonesfield Slate, which attracted the notice of Cuvier and many contemporaries from 1824 onwards; and it was Owen who gave the first extended account of the Purbeck Mammalia (collected by Mr. Beckles) in his *Monograph of the Fossil Mammalia of the Mesozoic Formations* (Palæont. Soc., 1871). The only known Lower Mesozoic mammalian skull was also described by Owen from the Karoo formation of Orange Free State in 1884 (*Tritylodon*). The Cetacean fossils from the Red Crag formed the subject of a brief *Monograph of the British Fossil Cetacea from the Red Crag* (Palæont. Soc., 1870); and it must not be forgotten that Owen was the first to recognise the affinities of the archaic *Zeuglodon*.

Limits of space forbid more. These are merely a few of the more salient features of Owen's contributions to our knowledge of the Vertebrata. We need only add that it is very largely owing to the prompt manner in which he interested himself in, and dealt with every new fossil submitted to him, that the British Museum and the Royal College of Surgeons are almost unique in their rich collections of Vertebrate Palæontology.

A. Smith Woodward.

VIII.

The Restoration of Extinct Animals.[1]

AS the writer very truly remarks in the volume before us, the general public most certainly does not appreciate dry bones, more especially when they are found in the detached and frequently imperfect condition so common in the older formations of this country. With the laudable endeavour to excite a more general interest in the monsters of a former world, he has accordingly set himself the very difficult task of endeavouring to clothe them with skin and muscle, and to set their portraits, as thus restored, before the eyes of a wondering public. Needless to say, in such a task everything depends upon the skill of the artist and his knowledge of anatomy, and as we believe that Mr. Hutchinson is neither an artist nor an anatomist, such criticisms as we have to make on his restorations will apply chiefly to the draughtsman, who, we trust, is as pachydermatous, for the nonce, as the animals he portrays.

The idea of restoring extinct animals so as to present some semblance—we do not care to speculate how remote—to their original form, is no new one, dating at least as far back as Cuvier, by whom very fine restorations were made of the Palæotheres and Anoplotheres of the Paris Basin. Later on Buckland and Conybeare attempted the restoration of the Ichthyosaur and Plesiosaur, and, indeed, so successfully that their models now require but comparatively little modification to bring them up to date. In all these cases, as well as in that of the South American Megathere, the restorers had more or less nearly complete skeletons of the animals in question before them, and they, accordingly, could not well wander very far from the truth. The same remark will apply to the restorations which have been from time to time attempted of the various large mammals of the Pleistocene and Pliocene. When, however, we come to tell the tale of what has been previously done in the way of restoring the giant land reptiles of the Secondary epoch, we have nothing to record but disaster and confusion. This is due to the circumstance that it was formerly considered possible to restore an animal correctly from a single limb or bone. Now, however, we know better, and are fully

[1] "Extinct Monsters. A Popular Account of some of the Larger Forms of Ancient Animal Life." By Rev. H. N. Hutchinson. With Illustrations by J. Smit. London: Chapman and Hall, 1892. 8vo. Pp. xx. and 254. Price 12s.

aware that the powers of the anatomist—great as they undoubtedly are—are not equal to the task of making anything approaching a correct restoration from such materials.[2]

For instance, what sort of a creature would have been the result of attempting to restore the Macrothere from the foot-bones which were long supposed to be the only portions of the skeleton known, although its teeth and jaws lay all the time in the national collection—in a case half the length of the gallery from the one containing the former?

This overwhelming confidence in what the anatomist could be legitimately supposed to effect led to the grotesque so-called restorations of the Iguanodon and other Secondary reptiles set up many years ago at Sydenham, where, we believe, they still remain to delude an unsuspecting public. Needless to say, the creatures thus modelled in plaster were monsters in every sense of the word, and about as unlike anything that ever existed in heaven or earth as they well could be.

Now, however, *nous avons changé tout cela*; and all those who know anything at all about the subject are fully aware that it would be idle to think of restoring any monster of a totally extinct type without having, at least, its skull, the bones of both fore- and hind-limbs, and some portion—the more the better—of its backbone. In this country, and Europe generally, as a well-known palæontologist once remarked, we prefer (from necessity) to construct our Dinosaurs piecemeal—and nice little squabbles we sometimes get into over these aforesaid pieces; and it would accordingly, with one or two exceptions, have been long before we should have been justified in attempting their full restoration, even if this were ever possible. Fortunately, however, America has stepped into the gap, and given us Dinosaur skeletons by the dozen; so abundantly, indeed, they may almost be said to be a drug in the market. We have horned Dinosaurs, four-legged Dinosaurs, two-legged Dinosaurs, and mail-clad Dinosaurs, all and every one of which are creatures more like the phantoms of a dream than any we should naturally have thought of as denizens of this world of ours.

Hitherto we have known these marvellous creatures only by their skeletons; and although these are, perhaps, fully sufficient for the scientific student, yet it is quite clear that they do not arouse the enthusiasm of the public, by whom they are doubtless not "understanded." Now, however, thanks to our author and his artist, we have them depicted in at least some semblance of their original guise, the artist having taken the skeleton as his model in each case, and clothed it in flesh and skin as best he might.

When we take into consideration the extreme difficulty of this task, we cannot but think that the draughtsman has acquitted him-

[2] We were surprised to see the other day in the *Times* the statement that Sir R. Owen restored the Megathere from a single bone

FIG. 1.—RESTORATION OF *Triceratops*

self well, and more than well; and any criticisms that we may have to make must be looked upon rather as suggestions than as indicating a carping spirit.

One of the most striking restorations in the book is that of the Horned Cretaceous Dinosaur forming the frontispiece, which we are enabled to reproduce (Fig. 1). Now, so far as regards the head and body, there is little room for criticism, but the case is very different when we come to the limbs. In the first place, we are led to ask why the artist took for his model—as we are fain to suppose he did—an Ungulate Mammal instead of a Crocodile in his restoration of the hind-limbs. That is to say, we wish to know why the upper segment of the hind-limb (femur) is included in the common integument of the body, instead of being, as in the Crocodile, free. We should, indeed, have thought it much safer to follow the crocodilian model in this respect. Then, again, we have greatest doubts as to whether the creature ever had the prominent Ungulate-like heel with which it is represented, as, on turning to the figure of its skeleton on page 166, we find there is no backward projection of the calcaneum, so that here also the departure from the crocodilian model seems unjustifiable. In the fore-limb it is, further, quite evident that the joint which appears intended to represent the wrist is placed much too high up; and we also doubt if any reptile could possibly have had the "action" in the fore-limb which the forward bend of this joint on the right side is clearly intended to represent. In all these points the want of an anatomical knowledge on the part of the author wherewith to check the exuberant fancy of his artist is only too apparent; and the former would certainly have been well advised had he submitted the sketches to some person well versed in the osteology of reptiles before having them photographed.

We may remark here that the author calls the reptile in question by the name *Triceratops*, but he should have been aware that this is certainly not its proper title; and we notice all through the book a lamentable want of care in this respect, the names assigned by one particular palæontologist to the animals figured being taken without the least enquiry as to whether they are correct. We should have thought, moreover, that in a popular work the insertion of specific names in the case of these giant reptiles was perfectly unnecessary, and only too likely to make it repellent to the unscientific public. Then, again, the number of generic terms introduced into the text is, to our fancy, far too numerous—more especially when many of them are probably synonyms. For instance, it would have been far better to allude to the skull figured on page 79 as that of a Carnivorous Dinosaur, rather than as *Ceratosaurus*, seeing that both Professors Cope and Baur are confident that it belongs to *Megalosaurus*. The author is also, in some cases, somewhat careless as to sources from which he derives his figures, and we may remind him that the skeleton figured on page 75 is *not* after Marsh.

While on the subject of Dinosaurs, we must take exception to the author's throwing any doubt (page 62) on the relationship existing between these reptiles and birds, which we venture to consider one of the best established zoological facts. Few, however, at the present day, would be disposed to assert that the Ratite birds (p. 63) —*qua* Ratites—are the direct descendants of Dinosaurs. The author is still more "flabby" with regard to the relationship between Dinosaurs and Crocodiles (p. 63), which is likewise a well-established fact; and if his own want of anatomical knowledge did not permit his perceiving their connection, he might at least have followed those who are entitled to speak with some authority on the subject. Instead of this, he goes out of his way to say that, while such connection may be "quite possible, and even probable," it cannot be considered certain.

To resume our survey of the plates, the restorations of the Brontosaur and Megalosaur (pls. iv., vi.) appear to us decidedly more successful than that of the Horned Dinosaur; although in the Megalosaur the fore-limbs are not quite satisfactory. In both these figures the objectionably prominent heel given to the Horned Dinosaur is wanting. As regards the Iguanodon (pl. vii.), it appears to us unnatural that, while the elbow-joint is represented as completely free from and below the body, the thigh is almost entirely enclosed in the integument; and we have also doubts whether the creature was really so short-legged as it is represented. When we contrast with it the figure of the Scelidosaur (pl. viii.), we are, indeed, surprised to find such a marked difference made between the two animals in these respects, and we are certainly not prepared to admit that, while the one was digitigrade, the other was plantigrade. Anyway, the different position of the elbow-joint in the two forms clearly shows that at least one of the restorations must be incorrect in this particular. With regard to the restoration of the armour in the Scelidosaur, we are aware that the artist has followed an attempted reconstruction of the skeleton, but, nevertheless, the huge spines on the shoulder-blades do not appear altogether natural. In putting the creature in an upright posture, we have, however, every reason to believe that the artist, in spite of certain criticism to the contrary, is fully justified. Judging from the skeleton of the so-called Stegosaur, we are inclined to think that in the restoration (pl. ix.) the hind-quarters and limbs do not give any adequate idea of their immense height and power as compared with the fore-parts.

Of the other Secondary reptiles, such as the Pterodactyles, Plesiosaurs, Ichthyosaurs, and Mosasaurs, the restorations appear, on the whole, as satisfactory as the materials on which they are based will admit. It was, however, unfortunate that the specimen of an Ichthyosaur referred to in an appendix was not described in time to admit of its forming the basis for the plate of that group. In treating of the Plesiosaur on page 50, we think it might have been well to

modify the statement that it was so named "in order to distinguish it from the *Ichthyosaurus*, and to record the fact that it was more nearly allied to the lizard than the latter." What exact signification the author may attach to the term *the lizard*, we are, of course, quite unaware, but the sentence as it stands is certainly misleading to the uninstructed. Again, on page 58, we are totally at a loss to imagine what special resemblance the Plesiosaur presents to "the strange Ornithorhynchus" (with which term, of course, all the readers of the book would be perfectly familiar!). If the author had written Ichthyosaur instead of Plesiosaur, there might have been something in the statement.

Before leaving the subject of reptiles, we should like to know why the author retains the name *Colossochelys* for the giant extinct Indian Tortoise, seeing that it has been conclusively shown to be inseparable from *Testudo*; and we also think he would have been better advised had he made no mention of the grossly-exaggerated restoration of its shell which still disfigures the fossil reptile gallery of the British Museum. Indeed, the whole fable of the mythological Indian tortoise, which Falconer sought to identify with this fossil, might well have been consigned to the oblivion it merits.

Two plates are devoted to birds, the one representing the toothed *Hesperornis* of the North American Cretaceous, and the other two of the New Zealand Moas. In representing *Hesperornis* with a steganopodous foot, that is to say, with the hallux connected with the other toes by a web, the artist has certainly no justification, this feature being at variance with the colymbine affinities of the genus. Here, then, we again meet with an instance where the author's want of acquaintance with the most ordinary facts of zoology has rendered him unfit for the task of supervision. We do not, of course, mean to assert definitely that this bird may not have had all the toes joined by membrane; but since its osteology is so close to that of *Colymbus*, it is probable that the hallux was separate, and it should have been restored accordingly. That the author adopts the view that the extermination of the Moas took place during the period that man has inhabited New Zealand, is evident from his plate; we regret, however, to find that he has not seen fit to follow the view of all living authorities on the subject that the two species of these birds selected for illustration belong to widely different genera.

In regard to mammals, we have not much to remark. One of the most striking illustrations is that of the Glyptodont in plate xviii. (herewith reproduced), where the proper form of the tail is given. Readers of the work are, however, likely to be somewhat puzzled on finding another member of the same genus depicted on page 174 with a totally different tail; and in common fairness they ought to have been informed that in this case the tail belongs to a totally different animal from that to which the carapace pertained. We do not, more-

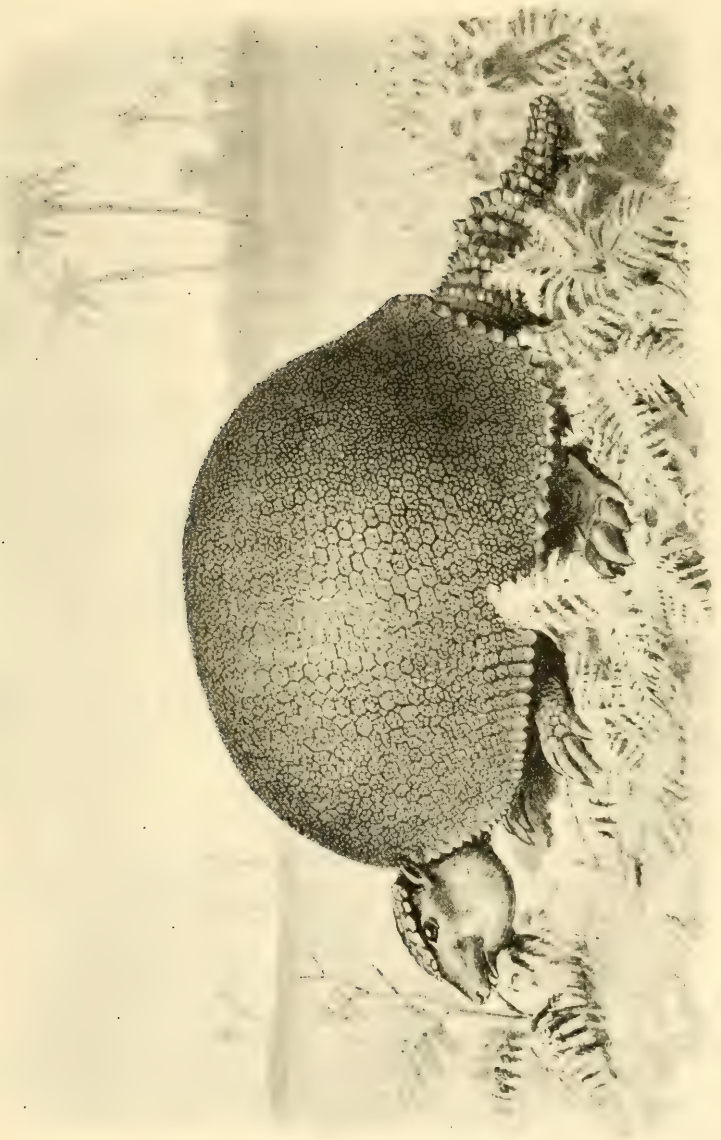


FIG. 2.—RESTORATION OF *Glyptodon*.

over, quite like the term armadillo applied to the Glyptodonts, especially as its original signification implies diminutive size; and surely, in face of the multitude of uncouth terms used by the author, there could have been no objection to the use of the name Glyptodont for these animals. The Mammoth is rightly restored on the lines of its near relative, the Indian Elephant, but surely the artist might have selected a better example of the latter animal as a model than the ill-bred, long-limbed, and small-bodied brute he has depicted. Why, for example, did he not take the figure of a "Koomeriah" Elephant from G. P. Sanderson's "Wild Beasts of India"? On page 199, the author enters into a comparison between the habits of the Mammoth and the African Elephant, and infers that the former uprooted fir-trees for food. He forgets, however, that the African and Indian Elephant have quite different teeth and quite different food, and that the Mammoth resembled the latter in the structure of its molars. Bearing in mind the great external difference between the Indian and African Elephant, of which no indications are given by their skeletons, it must be admitted that the restoration of the American Mastodon is a mere fancy sketch; and we quite fail to see the advantage of attempting the restoration of a member of any existing genus of animal unless its exact specific characters are, more or less, exactly known. The restoration of the Woolly Rhinoceros (which the author, as usual, calls by its wrong scientific name) appears to be rightly modelled, so far as the head is concerned, on the lines of the African Square-mouthed Rhinoceros. The artist has, however, made the beast "square-mouthed" with a vengeance, and has put the front horn too far back; and he would have done better if he had kept close to the figure of the head of the living species he drew a few years ago for the *Proc. Zool. Soc.* Moreover, the beast is too low at the withers, and has an altogether "sheepish" expression, which does not convey the proper idea of such a magnificent monster.

In the Indian Sivathere depicted in plate xvi., we note that the tips of the antlers are represented as curving forwards and inwards, whereas in the figure of the skull on the page facing the plate their direction is backwards and outwards. The restoration of the Uintathere (miscalled *Tinoceras*) in plate xiv., and that of the Titanothere (wrongly called *Brontops*)[3] in plate xv., appear to be fairly satisfactory, the feet of the one being formed on the Proboscidian, and those of the other on the Rhinocerotic model, while both have rhinoceros-like heads. Why both the plates should be lettered "new" extinct quadrupeds, we rather fail to see; and we had hoped the latter term had received its quietus.

[3] The author ought to be aware that, if he admits such terms as *Brontops* and *Tinoceras* to generic rank (if, indeed, they are not synonyms pure and simple), he has not the shadow of justification for including the Woolly Rhinoceros in the genus of that name.

On the whole, we think that, considering the difficulties of the task before them, both author and artist have produced a very creditable volume, and one which will not fail to arouse a considerable amount of popular interest. Should a new edition be called for, we think the author would do well to be somewhat less free in his use of uncouth and unfamiliar terms (*e.g.* heteroclite, p. 35) and technical names. Moreover, in many places his literary style and the conformation of his sentences might be altered for the better. For instance in a half-page paragraph of six sentences (p. 18) we find no less than three of these beginning with the word "But" and two with "In"; while the clumsiness of some of the sentences themselves would be rather hard to beat.

SOME NEW BOOKS.

INTRODUCTION TO PHYSIOLOGICAL PSYCHOLOGY. By Dr. Theodor Ziehen. Translated by C. C. Van Liew and Dr. Otto Beyer. 8vo. Pp. 284. London: Swan Sonnenschein & Co., 1892.

THE original German work, of which this is a translation, forms a useful introduction to the subject. It is short, it is written vivaciously, it is usually clear and well-arranged, always interesting; it is not a compilation, but the work of a man who has thought out the whole subject for himself. The doctrine is by no means free from exception, but it is set forth vigorously and consistently. In the present state of the subject, any short book is almost necessarily dogmatic in character. Perhaps the few polemical passages were better omitted altogether, as likely to create an unfair impression when not supported by proofs.

Professor Ziehen belongs to the small but able body of German psychologists who follow the model of the English psychology of association, and seek to improve upon their model. The latter half of the book attempts to show how associative processes account for the complexity of all mental phenomena above sensation. But the chief value of the work lies in the consistent and thoroughgoing attempt to exhibit in detail the physiological processes which underlie the psychical. Without this procedure, hypothetical as it is at present, we are unable to explain mental acts; and at the same time this method clears up many difficulties, such as those of unconscious mental states.

Professor Ziehen proceeds upon the hypothesis that ideas are deposited in different elements of the cortex from the corresponding sensations, though he is well aware that another hypothesis is possible. Unfortunately, in the very chapter on the association of ideas, his usual clearness deserts him. He does not explain clearly the relation of association by contiguity to association by similarity, and he is confused in dealing with the difficult question of whether the first kind of association is simultaneous or successive. This chapter is one of the less satisfactory parts of the book. His classification of actions into reflex, automatic (he uses this unhappy word in yet another sense) and actions proper, is well worked out, but it presents many difficulties. He has to class instincts along with reflex and automatic acts (*i.e.*, reflexes modified by intercurrent sensations—why sensations?) as purely unconscious. The first half of the book is an account of the various sensations, and of generalisations, like those of the specific energy of nerves and of Weber's law. A fuller account of colour-blindness and of Hering's hypothesis would be an advantage. The psychophysical methods are treated very inadequately, even making allowance for the difficulty of discussing them in a short book; and the treatment of the method of right and wrong cases (which the translators, following Professor Ladd, absurdly call the method of correct and false (mistaken) cases) is apt to be misleading. Still, on the whole, the selection made from the multitude of data is a good one for elementary purposes, and the statement is clear,

with one or two exceptions (*e.g.*, the notice of Cheselden's patient, on p. 81, and the account of Goldscheider's investigations of the sense of motion, p. 69).

A useful and able book like this, if it was to stimulate the growing interest taken in the subject in England, needed to be very well translated. Professor Ziehen has been unfortunate in his translators. They have obviously taken pains, but they do not appear well qualified for the task, either by complete mastery of the English language, or by close familiarity with the subject itself. They have made the initial mistake of abandoning the fluent, personal, lecture form of the original, for a buckram, awkward, impersonal form. The translation is, as a whole, clumsy and disfigured by inelegancies, and one is always reminded that the work is a German one. There are many strange words, such as taction, obtusion, appertinent, respective, literature, innerved, impeditive ("a very impeditive sheath of caoutchouc" for "a close-fitting indiarubber tube"), synchronically, sensual for sensuous (*e.g.*, "sensual vivacity"), to dampen for to damp. As Goldsmith at a famous dinner-party said of the phrase "happy rebellions"—we have not the phrase. The word "impart" is used freely; a stimulus "imparts" a sensation, one sensation even "imparts" another; a cortical centre "imparts" a motion—where produce or discharge is the accepted term. Magnitudes are "projected" on an axis of abscissas, instead of being "measured off." Very often the particles get wrong, and the order of words destroys the emphasis. On page 124, by translating "höchstens" by "at least" instead of "at most," they make the passage unintelligible, and many other such confusions occur (*e.g.*, on pages 215, 245, 44.).

More serious mistakes arise from unfamiliarity with the subject. Awkward or unusual names are used, gustatory bulbs (instead of taste-buds or taste-bulbs), zone of Rolando for the motor area, threshold of distinction (which should surely be "difference"), faradic electricity, synovial duplicatures, auditory bones. In some cases the translators improve on their author, and make him blunder; the description of chemical stimuli on p. 37 is absurd in the translation; on page 102 they blunder gratuitously over the connection of the hemispheres with the different sides of the field of view; in the same chapter the nodal point of the eye is described as the point at which the rays (*i.e.*, from a single point) intersect, not a word of which of course, is in the original. The "well-known blind patient of Cheselden" is actually rendered "the well-known Cheseldens, who was born blind." It is amusing to find, in a work intended for English readers, "the then youthful English statesman, Gladstone." The mistakes occur principally in the earlier, more physiological, half of the book; the later half is fairly readable, but it, too, contains both blunders and inelegancies. On the whole, the translation must be pronounced very unsatisfactory, and contrasts glaringly with the excellent translation of Höffding's "Outlines," which is a boon to the English student. It speaks much for Professor Ziehen that he is interesting in spite of his translators.

S. A.

Public Health Problems. By John F. J. Sykes, B.Sc., &c. [Contemporary Science Series.] London: Walter Scott, 1892. Price 3s. 6d.

The peculiarity of this book is, that the author, to use his own words, has attempted "to cast a reflective and suggestive line of thought

into the volume." With this idea before him, he deserts the ordinary empirical method of the text-books, and attempts to group the "Problems" and the Ideas which are prevalent in Biology at the present time. In the later part of the book, especially in Part III., where the author falls back on the Empirical method, and takes up, in this fashion, subjects such as "Quarantine," "Isolation," &c., we have him at his best. Here he discusses the subjects in an entirely practical manner, and does not attempt to justify his conclusions on other than practical grounds.

It is with the earlier part of the book that we are inclined to find fault. The conception of evolution has hitherto not been found of much service in the sciences of Physiology and Pathology. It is chiefly of value to Morphologists. To try to introduce it into Hygiene, which depends on Physiology and Pathology for the basis of its doctrines, is a much more serious matter than can be undertaken in a small text-book. The chapter on "Heredity," for example, is not very conclusive. The author tells us that in Pathological changes the organism passes beyond the limit of Physiological adaptation. In this fact we are to find the origin of Disease and Death. The extreme character of Pathological modifications produces such an effect on the germ-plasm that they are inherited more readily than Physiological modifications (13). To supplement this position, which he does not prove, he tells us how we may hope to obtain "amelioration in the fitness of the generations yet to be born." Sufficient knowledge is to be imparted to the rising generation that when the time comes the members of it may select mates so as "to counteract injurious or to supplement deficient characteristics." "Indirect as it may appear, the individual possesses distinct power of adaptation over the offspring. The weakness of a system, such as the nervous or respiratory, in the one parent may be counteracted by the other, and any neglect to take cognisance of such a weakness on both sides more surely results in disease in the children" (25).

How is this "counteraction" brought about? It is surely not by both sides taking cognisance the neglect of which is here blamed. The author's belief in education is unbounded, and leads him to the following extraordinary conclusion: "For the future citizen the earliest teaching of the school must be how to live healthily and have healthy offspring," p. 28. The rest of Part I. claims more or less similar criticism. The style in which it is written is awkward, as if the author were unfamiliar with the subjects. Occasionally, as in the account of carbonic acid, the author shows himself unacquainted with recent work upon the questions at issue.

ETHNOLOGY IN FOLK LORE. By G. L. Gomme. ["Modern Science" Series, edited by Sir John Lubbock.] 8vo. Pp. 200. London: Kegan Paul, Trench, Trübner & Co., 1892. Price 2s. 6d.

THIS volume, by the president of the Folk-Lore Society, aims to set forth the principles upon which the subject may be classified, in order to arrive at some of the results which should follow from its study. Old races disappear while old customs last—carried on by successors, but not necessarily by descendants. Many customs and beliefs exist uselessly in the midst of civilisation, but their true meaning may be gathered if they can be traced to other countries where they occur in harmony with the manners and ideas of the people. The author maintains that the records of uncivilisation are as real as those of

civilisation, and that both belong to the same geographical area. Historians often ignore the less pleasing of the two records, and magnify the more pleasing. For instance, the records of life in various parts of London at the present day are painfully different, so are those in many parts of the Hebrides. Some of the conclusions of the author may be startling to those who have given no attention to the subject. He says:—"It would appear, then, that cannibal rites were continued in these islands until historic times; that a naked people continued to live under our sovereigns until the epoch which witnessed the greatness of Shakespeare; that head-hunting and other indications of savage culture did not cease with the advent of civilising influences, that, in fact, the practices which help us to realise that some of the ancient British tribes were pure savages, help us to realise also that savagery was not stamped out all at once and in every place, and that, judged by the records of history, there must have remained little patches of savagery beneath the fair surface which the historian presents to us when he tells us of the doings of Alfred, Harold, William, Edward, or Elizabeth." We wonder why the author refrains from adding that "little patches of savagery" continue to manifest themselves alongside of the "advanced guard of the nation."

MAN AND THE GLACIAL PERIOD. By Professor G. Frederick Wright. [International Scientific Series.] Pp. 385. London: Kegan Paul, Trench, Trübner and Co., 1892. Price 5s.

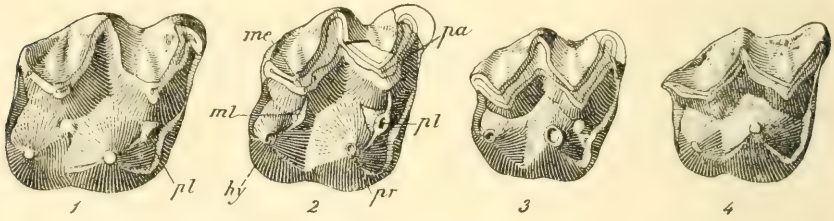
THE new volume by Professor Wright is somewhat disappointing. Another book has been added to the fast-growing literature of the Glacial Period, but we cannot feel that any real advance has been made in our knowledge of the subject, or that the author has even given us a good *résumé* of what is already known. No doubt to most European readers the account of the American Glacial deposits will be new; but so many of the statements made have been challenged by competent American authorities, that we should hesitate to recommend the book. In face of the explicit repudiation of all responsibility by the United States Geological Survey, of which Professor Wright was formerly an assistant, it will be safer to wait till a larger area has been properly examined.

The title of Professor Wright's book scarcely leads one to expect that less than a sixth of the volume has anything to do with the antiquity of man, the rest being taken up with stock subjects, such as glaciers and glacier motion, ancient glacial deposits of various parts of the world, the cause of the Glacial Period, &c. If this section were well done, we should not object, but it shows an imperfect knowledge of the literature, and an inability always to select good authorities for districts with which the author is not personally acquainted. As to the genuineness of the mortars, and the clay image, stated to have been found at great depths in the Western States, we prefer to suspend our judgment till American geologists are satisfied. We have had a considerable experience of the miscellaneous articles said to be found in mines and quarries, and without in the least suggesting bad faith on the part of the finders, we may remind our readers of the living toads discovered in coal, of the horse-bones or garden snails found in the Chalk, and of the miscellaneous articles unintentionally dropped down deep borings, to be brought up again by the boring tool.

A MEMOIR ON THE GENUS *Palæosyops* AND ITS ALLIES. By C. Earle. Extract from Journ. Acad. Nat. Sci. Philadelphia, vol. ix., pp. 267–388, pls. x.–xiv. (1892).

WE are glad to welcome as a comparatively new worker in mammalian palæontology Mr. Charles Earle, who in the finely illustrated memoir before us has produced a monograph which will form a fitting companion to those written by Professors Scott and Osborn on other groups of Ungulates. The systematic and methodical work now being undertaken by these three gentlemen on the fossil mammals of the United States is of the highest value and importance, since by this means alone can American vertebrate palæontology be rescued from the confusion with which it is beset through the over-zeal of describers anxious to procure priority for their own names.

The group Mr. Earle has set himself to monograph is one of peculiar interest, since it forms part of a family of Perissodactyle Ungulates which has no European representatives. *Palæosyops*, it may be observed, included animals of about the size of a tapir, with a skull of somewhat similar type, and the same number of digits to the feet. They had, however, upper molar teeth of a totally different type, which is described by Mr. Earle under the name of *buno-selenodont*; that is to say, while the inner pair of columns formed simple cones,



Right Upper Molar Teeth of *Palæosyops* (1, 3), *Limnohyops* (2), *Telmatotherium* (4), *pa.* paracone; *me.* metacone; *pr.* protocone; *hy.* hypocone; *pl.* protoconule; *ml.* metaconule.

the outer pair were flattened and crescent-like. Molars of almost identical structure occur in the European genus, *Chalicotherium*.

Palæosyops, together with some nearly allied forms to which distinct names have been applied, occurs in the upper portion of the Middle or Bridger Eocene; while in the Lower Bridger they were preceded by a nearly allied but more generalised form known as *Lambdotherium*, which was probably very close to, if not the actual ancestor of the group. These forms are characterised by possessing the full typical dentition,[1] and by the upper premolar teeth being less complex than the molars, while the skull was devoid of bony horn-like appendages, and the third trochanter of the femur fully developed. Professor Cope regarded these forms as constituting a family by themselves; this view was, however, disputed by Dr. Schlosser, who proposed to include them in the same family with the larger forms from the Miocene, known as *Titanotherium*.

The latter view is supported by Mr. Earle, who shows that the two groups can only be distinguished by the circumstance that *Titanotherium* and its allies have at least some of the upper premolars as complex as the molars. In the gigantic *Titanotherium* of the Miocene, the skull was provided with enormous bony horn-like

[1] In the one named species of *Lambdotherium*, the first lower premolar is absent.

appendages, doubtless sheathed in true horn during life; several of the upper premolars were as complex as the molars, the incisor teeth were reduced in number or wanting, and the femur had nearly lost its third trochanter. *Diplacodon*, of the upper or Uinta Eocene, forms, however, as might have been expected from its geological horizon, a perfect connecting link between *Titanotherium* on the one hand and *Palæosyops* and its allies on the other. In this genus the skull has no horn-like appendages, while only the last of the upper premolars resembles the molars, and the typical three pairs of incisors are retained.

The evolutionary development of the *Palæosyops-Titanotherium* line may, therefore, now be regarded as worked out as fully as that of the Rhinoceroses, and it is curious to notice how closely the two groups follow parallel courses in this respect. Thus, in both there has been a development of horn-like appendages to the skull; in both the premolars have tended to assume a molariform structure, while in both the last upper molar has more or less completely lost the hinder of its two inner columns; then, again, the more specialised forms in each group have the front of their jaws edentulous. On the other hand, while the most specialised Rhinoceroses have acquired high-crowned (hypsodont) molar teeth, and have reduced the number of digits on the fore-limb from four to three, the Titanotheres have still low crowned retained the (brachyodont) molars and tetradactylous fore-feet of their Eocene ancestors.

So far as we can gather from his memoir, Mr. Earle seems to consider that the unguiculate *Chalicotherium*, in spite of the similarity in the structure of its molar teeth, has no sort of affinity with *Palæosyops* and *Titanotherium*. This, however, we are not at present prepared to admit. R. L.

The Student's Handbook of Physical Geology. By A. J. Jukes-Browne, B.A., F.G.S. 2nd edition. Pp. 666. London: George Bell & Sons, 1892. Price 7s. 6d.

The new edition of Mr. Jukes-Browne's Handbook is too bulky, and would be improved by judicious compression. We would suggest, for instance, that it is quite unnecessary to say so much about the internal state of the earth, considering how little is known, and how likely to mislead is an appearance of knowledge. Already the dogmas of the rigidity of our globe and the stability of the earth's axis have been rudely shaken, and even the mathematicians hesitate. Is it really necessary, also, to ask the unfortunate student to master such terms as *metatropy*, *metataxis*, and *metacrasis*, and to understand what they mean? No examiner is likely to expect a knowledge of a number of technical expressions which he does not himself use, and which are not current coin.

We draw attention to these slight imperfections, for it is evident that the book meets a want, and will have an established place as a class-book for students, if it can be kept within reasonable limits as to size and price.

Atlas des Algues Marines. By Paul Hariot. Pp. 51. 48 Plates. Paris: Klincksieck, 1892. Price 12 francs.

This volume of the *Librairie des Sciences Naturelles* aims at guiding the young phycologist to a knowledge of the more common seaweeds. On the 48 plates there are represented 110 species (not 108 as stated on the title-page), easily collected on the shores of France, and, with

one or two exceptions, it may be added on the shores of Britain. The plates are photographic reproductions of dried and mounted specimens, and are printed in appropriate colours (green, brown, and red). They will certainly be a greater help to the student of sea-weeds, after these have been dried, than to the student of them who begins by trying to name the plants fresh from the pools. This is inevitable, however, and nearly every botanist has had the experience of being utterly puzzled by a plant in the field or garden which, on being dried, has revealed itself as an old herbarium friend. The text consists of excellent directions for collecting and preparing specimens, and of short and good descriptions of the Algæ figured, with a glossary of the few unavoidable scientific terms used. M. Hariot is a phycologist of distinction, and has brought accuracy and good sense to his task. Though the plates are not things of beauty, they are sufficient for their purpose, and the choice of species figured is good, in the greatest number of cases, for Britain. It will prove useful to beginners in this country who are familiar with French.

English Botany. Supplement to the Third Edition. Part III. (Orders xxvi.—xl. Compiled and illustrated by N. E. Brown. London: George Bell & Sons, 1892. Price 5s.

Parts I. and II. of this Supplement have already been noticed; Part III. brings us to the end of Dipsaceæ, and includes also the remainder of Rosaceæ, Onagraceæ, Cucurbitaceæ, Crassulaceæ, Saxifragaceæ, Umbelliferæ, Rubiaceæ and Valerianaceæ. Mr. Brown's connection with the work ceases with the present number, its completion falling to the lot of Mr. Arthur Bennett. With Part III. is published the preface, in which the scope of the work and the rules adopted in nomenclature are explained; no authorities for genera are accepted that date farther back than the year 1735, when Linnæus published the first edition of his "Systema Naturæ." Authorities for species do not date farther back than 1753, when Linnæus established the binomial system in the first edition of his "Species Plantarum." "It seems an act of gross folly," says the writer, "to apply the binomial system—as has been attempted in America—to dates before that system was in existence," and we heartily agree with him.

We also commend the expression of his view on the hybrid-making epidemic. No less than seven and twenty names follow the genus *Epilobium*; they are headed "Hybrids?" and "are supposed to be natural hybrids, and are considered as being intermediate in character between their supposed parents." The differences, however, "between the supposed hybrid and the species it most resembles being no greater and sometimes not as great as may often be found between individuals in a bed of seedlings from one plant." Mr. Brown sees no use in inserting in our floras descriptions of such plants. Coloured plates are given of *Pyrus rotundifolia* var. *decipiens*, *P. intermedia*, *P. pinnatifida*, *P. semi-pinnata*, *P. cordata*, and *Silenum curvifolia*.

Messrs. William Wesley & Son have issued a list of the transactions of Scientific Societies, Periodicals, and Serials, including a nearly complete enumeration of the various scientific journals that have at different times been published in Britain. It is interesting to note how few of those devoted to popular exposition have survived for more than two or three years.

OBITUARY.

JOHN OBADIAH WESTWOOD.

BORN DECEMBER 22, 1805. DIED JANUARY 2, 1893.

LAST month we chronicled the death of the veteran entomologist, Mr. H. T. Stainton, and now we have to record the passing away of a yet more venerable figure among British students of insect life, Professor Westwood, of Oxford.

Westwood's native town was Sheffield; there, and at Lichfield, whither his family afterwards removed, he was educated at private schools. A strong taste for natural history, a marked aptitude for drawing, a love for Lichfield Cathedral and its services, were characteristics of his boyhood, and all three bore fruit in his after-life. On leaving school Westwood was articled to a solicitor in London; he afterwards became a partner, but soon relinquished law to give himself to his chosen studies—entomology and ecclesiastical art. Known to naturalists throughout the world by his work on the former subject, he has acquired a wide reputation among archæologists by his descriptions and beautifully executed copies of ancient Christian MSS., and illuminations, ivories, and inscribed stones. His archæological work was carried on concurrently with his entomology, from the publication of the *Palæographia Sacra Pictoria* (1845) to the *Facsimiles of the Miniatures and Ornaments of Anglo-Saxon and Irish MSS.* (1868), the *Catalogue of the Fictile Ivories in the South Kensington Museum* (1876), and the *Lapidarium Walliæ* (1876-9). It is to his beautiful draughtmanship that we probably owe the combination in Westwood of two such apparently dissimilar lines of study as archæology and entomology.

His entomological writings date from 1827, when he began to contribute papers on various orders of insects to different journals and to the publications of the Linnean and other Societies. In 1837 he published a new edition of Drury's figures of exotic insects, for which he wrote descriptions. In 1838 appeared his *Entomologist's Text-Book*, to be followed in the two following years by the two volumes of the *Introduction to the Modern Classification of Insects*.

This latter work is one of the classics of British entomology, and no later work on insects generally, published in England, has been able to supersede it. Many of its speculations seem strange to naturalists reared in modern days, as the arrangement of the insect

orders in two "circular" groups, a mandibulate and a haustellate group of five each, in accordance with the theory of Macleay that all animal orders should fall into such an arrangement. Westwood was, however, careful to state that he did not regard the Macleayan scheme as final, but considered it as a step towards a truly natural arrangement; he did not adopt it in dividing the orders into tribes or families. The *Modern Classification* was illustrated by careful figures of the distinctive parts of representative insects of each family or important genus, and is specially valuable in containing figures of larvæ, for which the student looks in vain in most recent systematic works on single orders, except those on the Lepidoptera and Hymenoptera.

In 1841 appeared the *British Butterflies*, to which work Westwood contributed the descriptions and Humphreys the plates. A similar association of the two authors produced *British Moths*, in two volumes, in 1843–5. In the latter year appeared also Westwood's *Arcana Entomologica*, in two volumes, containing descriptions and exquisite illustrations, by himself, of new and rare insects; the *Cabinet of Oriental Entomology*, also remarkable for the beauty of its plates, appeared in 1848. Westwood's next great work was the *Genera of Diurnal Lepidoptera*, in which he was associated with Doubleday and Hewitson; the latter was responsible for the illustrations to both volumes, Doubleday wrote the first, and Westwood the second, which was published in 1850–2, and comprised part of the Nymphalidæ, the Brassolidæ, Satyridæ, Libythæidæ, Erycinidæ, Lycænidæ, and Hesperidæ. This monograph was a sterling contribution to the classification of butterflies, and reduced to order the chaotic assemblies of species of earlier writers. About this time, Westwood drew the illustrations for Walker's unhappy work on the *British Diptera*. The Royal Society bestowed on him a Royal Medal in 1855. In 1859 appeared his *Catalogue of Phasmidæ in the British Museum*, an excellent monograph of that most interesting group of Orthoptera.

Westwood's connection with Oxford now began. In 1858, Rev. F. W. Hope presented to the University a valuable collection of insects, including some purchased from Westwood, who was appointed curator of the Hope Museum, which had thus been formed. The University was at that time without a Professor of Zoology; the munificence of Hope endowed a chair, to which he was to make the first nomination, and, in 1861, he appointed Westwood.

The new Professor, who had had no University career, received an honorary M.A. from Oxford, and was introduced to Magdalen College, of which, in 1880, he became an Honorary Fellow. For more than thirty years, therefore, he has been a familiar figure at his adopted University, of which, at the time of his death, he was probably the oldest resident. Shortly after his appointment he was associated with Mr. Spence Bate in a monograph of the *British Sessile-*

Eyed Crustacea, one of his very few zoological works beyond the limits of the Insecta. At this time the zoological world was convulsed with the controversy which followed the publication of the *Origin of Species*. In the year that Westwood was appointed to his chair, the British Association met at Oxford, and the famous debate between Professor Huxley and Bishop Wilberforce took place. Westwood, like most systematic naturalists of his day, was strongly opposed to the new views; he attacked the Darwinian theory in a few papers and letters, but prolonged controversy seemed distasteful to him. He went on steadily with his systematic work, and, while he continued to the end firm in his convictions, he saw ere his death a biological school established at Oxford whose teachers owe their inspiration to the work of Darwin. He lived, also, to see Oxford theologians welcome evolution as a valuable contribution to religious thought.

The collection under his charge at Oxford, second only to that of the National Museum—and in some groups superior—now occupied his attention, and in 1874 the new and rare insects of the Hope Museum were described and figured in the magnificent work known as the *Thesaurus Entomologicus Oxoniensis*. In the same year he issued a second edition of the *Butterflies of Great Britain*, in which the plates, as well as the text, were from his own hand. In the last few years of his life he returned to the study of the Orthoptera, and published, in 1889, his final great work, the *Revisio Insectorum Familiæ Mantidarum*.

Besides the books enumerated, numberless papers on entomological subjects by Westwood have appeared in scientific journals and transactions during the last sixty-five years. His contributions to the science of insect-life and structure were confined to no special group; the literature of every order has been enriched by his researches. Nor did he confine his labours to the purely scientific aspect of entomology; in many articles contributed to the *Gardener's Chronicle* he elucidated the history of the insect enemies of the cultivator of the soil. At the present time, when extreme specialisation threatens to become more and more the habit of systematic naturalists, it is well to remember that Westwood has shown that it is possible to do a vast amount of thoroughly good work without unduly narrowing the range of study. To his admirable personal qualities, all who had the pleasure of his acquaintance bear hearty witness.

G. H. C.

JOHN STRONG NEWBERRY, M.D.

Born December 22, 1822. Died December 7, 1892.

A PIONEER and honoured leader in North American Geology has just passed away in the person of Professor Newberry, of New York. Born seventy years ago at New Windsor, Connecticut, and removed in early childhood to the then newly-founded Cuyahoga Falls City, in Ohio, he was educated for the medical profession,

graduating in 1846 at the Western Reserve College, and later at the Cleveland Medical College in 1848. In 1849–50 he travelled in Europe, and in 1851 entered upon private practice as a physician at Cleveland, Ohio.

Newberry, however, was imbued from childhood with a taste for Natural Science, and the routine of a medical practitioner soon proved irksome. In 1855 he determined to devote himself to purely scientific investigation, and during that year he undertook the duties of surgeon, geologist, and naturalist to the Government expedition under Lieut. Williamson, organised to explore the country between San Francisco and the Columbia River. This resulted in his first published work on *The Geology, Botany, and Zoology of North California and Oregon*. In 1857–58 Newberry was one of the original explorers of the now well-known cañons of the Colorado River, and, still later, he prosecuted geological researches in New Mexico, Arizona, and Utah.

Newberry's distinguished services were soon rewarded, and in 1866 he became Professor of Geology in the School of Mines, Columbia College, New York. Two years later he also received the appointment of State Geologist of Ohio, and thenceforward his more systematic work began. The reports of the Geological Survey of Ohio, published under his direction, contain not merely stratigraphy and economics of local interest, but also detailed monographs on palæontology of fundamental importance. The descriptions of the Devonian and Carboniferous fishes and plants were contributed by Newberry himself; and these, with subsequent writings on the same subject, led to an entirely new view of the gigantic "placoderm" fishes of the Palæozoic period. A large and unique collection accumulated, and this the Professor placed in his museum at the Columbia College.

While actively engaged in lecturing and teaching until two years ago, Professor Newberry continued his favourite studies of the Palæozoic and early Mesozoic fishes and plants with so much success that in 1888 and 1890 he was able to issue two great monographs under the auspices of the United States Geological Survey. The first volume relates to the Triassic fishes and plants of New Jersey and Connecticut, and the second deals with various Devonian and Carboniferous fishes, each containing a synopsis of earlier researches, combined with new figures and many observations not previously published. Though somewhat antiquated in their style of treatment of the matter, and not remarkably systematic, these two works will ever remain standards for reference, and the mine of clearly-enunciated facts they contain will form an enduring monument of the author's industry and acumen. Even until the last, Dr. Newberry was contemplating a supplementary volume on the Palæozoic fishes, and he also had nearly ready for issue a monograph on the Cretaceous flora of New Jersey.

Dr. Newberry was one of the founders of the United States National Academy of Sciences, and occupied the Presidential chair of the New York Academy of Sciences from 1867 until 1891. In 1867 he presided over the meeting of the American Association at Burlington. In 1883 he became a Foreign Member of the Geological Society of London, and in 1888 he received the Murchison Medal awarded by this Society. Whether in his quiet home at New Haven, or in the Museum of Columbia College, or wandering abroad, the privilege of meeting Dr. Newberry was one to be cherished. He was truly esteemed by all who came in contact with him, and the memory of his friendship will long be treasured, both by his pupils and fellow-workers.

THOMAS DAVIES.

Born December 29, 1837. Died December 21, 1892.

BY the death of Mr. Thomas Davies, Senior Assistant in the Mineralogical Department of the British Museum, Mineralogy in this country loses one of its most accomplished students. He was the son of the late Mr. William Davies, for forty years connected with the Geological Department of the same Museum, and began his career as third-class attendant under Professor Maskelyne in 1858. For several years Davies was the only member of the Museum staff deputed to assist the Professor in arranging the collection of minerals after its separation from the Geological Department, and he rapidly acquired the foundation of that remarkable knowledge of mineral species for which he became so noted in later years. In 1862 he was promoted to the rank of transcriber, and in 1880 he received the well-merited reward of appointment to a senior assistantship. Mr. Davies was a prominent member of the Mineralogical Society, acting for some years as Editor of the *Mineralogical Magazine*, and later filling the office of Foreign Secretary. Besides mineralogical notes, he published several contributions to the petrology of the older rocks, and in 1880 he was awarded the Wollaston Donation Fund by the Geological Society of London.

MARTIN SIMPSON.

Born 1799. Died December 31, 1892.

BY the death of Martin Simpson, of Whitby, at the advanced age of 93, Yorkshire loses one of its earliest geological explorers. He was a young man when Young and Bird published their "Geological Survey of the Yorkshire Coast" (1822); he was associated with Bean, Williamson, and John Phillips in their early work on Yorkshire geology; and later on rendered assistance to Tate and Blake. The Whitby Museum was established in 1823, and Simpson, in 1837, was

appointed Curator, also assuming duties as Lecturer on Natural Science to the Literary and Philosophical Society.

In 1843, Simpson published a short Monograph giving descriptions of more than 100 Ammonites of the Yorkshire Lias; and in 1855 he published descriptions of all the known fossils of the Yorkshire Lias, and an outline of the Geology of the Coast. A second edition of "The Fossils of the Yorkshire Lias" was published in 1884, while his "Guide to the Geology of the Yorkshire Coast" reached a fourth edition in 1868. As Simpson's descriptions of his fossils were not properly defined, only about 20 of his original species of Ammonites are now accepted.

In 1884 the Murchison Geological Fund was awarded by the Geological Society to Simpson, and he then remarked that he felt the more honoured as he had received no assistance or encouragement from his fellow-townsmen.

We also have to record the death of Benjamin Vetter, Professor of Zoology at the Polytechnic in Dresden; of the veteran Russian mineralogist, N. I. Kokscharow, author of the *Beiträge zur Mineralogie Russlands*; and of H. F. Blanford, F.R.S., late Chief of the Meteorological Department in India.

We are happy to state that the announcement of the death of Dr. D. Stur appearing in certain French and English papers, is founded on the mis-translation of a German paragraph. The late Director of the Austrian Geological Survey is now enjoying a State pension.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Professor F. W. Hutton, F.R.S., has been appointed curator of the Canterbury Museum, New Zealand, and Lecturer on Geology in the University College.

Mr. Walcot Gibson has been appointed an assistant geologist on the Geological Survey of Great Britain. He received his geological training under Professor Lapworth, and has since travelled in the regions of the Transvaal and Uganda. An important paper on the gold-bearing and associated rocks of the Southern Transvaal was communicated by Mr. Gibson to the Geological Society of London in 1892.

It is proposed to establish an Agricultural College for Kent and Surrey, under the name of the "South-Eastern Agricultural School and College." A scheme for the conversion of Wye College to this purpose has been submitted by the Charity Commissioners to the Committee of Council on Education.

The Colleges of Aberystwith, Cardiff, and Bangor are just completing the preparation of a draft charter for the proposed degree-conferring University for Wales. This was discussed at a conference at Shrewsbury on January 6, but it does not apparently meet with much favour among those best acquainted with the requirements of higher education in the Principality. While nominally founding only one University, some fear that it will raise each of the three constituent colleges independently to that rank.

The Council of the British Institute of Preventive Medicine is appealing for donations towards the cost of erection of a suitable building. The circular is signed by Sir Joseph Lister (Chairman), Sir Henry Roscoe (Hon. Treasurer), and Dr. Armand Ruffer (Hon. Secretary), and is accompanied by a long list of donations already received. The sum of £20,000 has been promised to the funds of the Institute by the trustees of the late Mr. Richard Berridge, on condition that a further sum of £40,000 is raised for land and buildings.

Under the presidency of H.R.H. the Prince of Wales, a committee has been formed to arrange for a suitable memorial of the late Sir Richard Owen. Sir James Paget, Sir William Flower, and Mr. W. Percy Sladen (Secretary, Linnean Society) have consented to act respectively as Vice-Chairman, Treasurer, and Secretary. A meeting was held in the rooms of the Royal Society of London, Burlington House, on January 21, when the first list of subscriptions was read, and it was resolved to place a full-length marble statue of the late Superintendent of the Natural History Departments of the British Museum in the hall of the Museum at South Kensington.

At the meeting of the Bristol Town Council on January 2, the gift of the Bristol Museum and Library was duly accepted on behalf of the citizens. The resolution was carried unanimously amid much enthusiasm.

A Naturalists' Field Club has been founded at Limerick, and has joined the other Irish Societies in adopting the *Irish Naturalist* as its organ.

The Cambridge Entomological Society has extended its scope so as to include other branches of Natural History, and will henceforth be known as the Cambridge Entomological and Natural History Society. The Society is not confined to members of the University, and at the present time is in a more flourishing condition than it has been for some years past. The secretary is Mr. W. Farren.

The medals and funds at the disposal of the Council of the Geological Society of London will be awarded as follows, at the Annual Meeting on February 17:—Wollaston Medal, Professor N. Story Maskelyne, and Fund to Mr. J. G. Goodchild; Lyell Medal to Mr. E. T. Newton, and Fund divided between Miss C. A. Raisin and Mr. Alfred N. Leeds; Murchison Medal to the Rev. O. Fisher, and Fund to Mr. G. J Williams; Bigsby Medal to Professor W. J. Sollas.

At the meeting of the Mineralogical Society of Great Britain on January 17 a biographical notice of the late foreign secretary, Mr. Thomas Davies, was read, with the announcement that a memorial fund would be collected for the benefit of the widow and family of the deceased. Professor Story Maskelyne is president of the committee, Dr. Hugo Müller is treasurer, and Mr. H. A. Miers has undertaken the duties of secretary. Donations may be forwarded to Dr. Hugo Müller, F.R.S., 13 Park Square East, Regent's Park, London, N.W.

A number of Conchologists residing in and around London have decided, if possible, to found a society devoted exclusively to the study of Mollusca, with its headquarters in the Metropolis. It is suggested that monthly meetings should be held from November to June, for the exhibition of specimens and the reading and discussion of papers, which it is intended should in due course be published and distributed to members. It is not proposed to form a library or a collection—at all events for the present. It is estimated that a subscription of 10s. 6d. per annum would be sufficient to effect these objects (no entrance fee being charged to those who join in the course of the first year). A circular has been issued by Mr. E. R. Sykes (of 13 Doughty Street, London, W.C.), who will be glad to hear from all who are willing to further the object in view. In the preliminary list of supporters we observe the names of Colonel Beddome, Lieut.-Colonel Godwin-Austen, Messrs. Cosmo Melvill, E. A. Smith, and B. B. Woodward, the Rev. R. Boog Watson, and Dr. H. Woodward.

CORRESPONDENCE.

Occurrence of Sowerby's Whale *(Mesoplodon bidens)* on the Norfolk Coast.

On December 19, 1892, I received a telegram stating that a "strange fish" was on shore at Overstrand, near Cromer, and subsequently that it was some species of Whale; on the 20th, in company with Mr. S. F. Harmer, of the Museum of Zoology and Anatomy, Cambridge, who happened to be staying in this neighbourhood, I went to Overstrand, and we found it to be an adult female of the above rare species. Its history, we learned, was as follows. At about 8 a.m. on Sunday, December 18, one of the Overstrand fishermen saw from the cliff an object lying in the water near the beach, which he at first took to be a log of wood, but soon perceived to be a "large fish." After obtaining assistance he fastened a noose over its tail, and secured it by an anchor, till it was placed on a trolley and drawn up the gangway to a shed on the cliff, where we saw it. The animal was alive when first observed, but died before it was taken from the water. As placed, it was, unfortunately, in such a position as to render photographing impossible, and our attempts proved unsuccessful; I believe no photograph was taken after it had been removed from the shed. Before our arrival it had been eviscerated, and a very advanced fœtus was taken from it. We made a very careful examination of the exterior, and hope to publish a full description in due course. In the meantime, I may say that the female was of a uniform glossy black colour, with the exception of the anterior edges of the flukes of the tail, and the jaws, which were grey, of various shades, in places almost white, and the body was spotted and blotched with white or pale grey in a very curious manner; the fishermen told us that when quite fresh out of the water there was a bluish shade pervading the whole. The young animal was black above, and reddish on the sides and lower parts, probably owing to the effusion of blood into the skin, which would doubtless otherwise have been white. The total length of the old female, measured in a straight line to the centre of the tail, was 16 ft. 2 in., and that of the young one 5 ft. 2 in.; across the flukes of the tail the adult female measured 3 ft. 8 in.

The present is the nineteenth known example of this remarkable animal, all of which have been met with in the North Atlantic during the present century, but with the exception of one taken in 1889 at Atlantic City, which came into the possession of the United States National Museum at Washington, and of which no account has, I believe, at present been published, in no other instance has an example in perfect condition come under the notice of a Cetologist. Individuals, or their remains, have been found in Scotland and Ireland, but the only previous English example was met with at the mouth of the Humber, in September, 1885.

T. Southwell.

Norwich, *January* 9, 1893.

[This specimen has been purchased for the Rothschild Museum, Tring.—Ed.]

Movement of Diatoms.

Two notices have appeared in Natural Science containing criticisms of my paper on the occurrence of "Pseudopodia" among the diatoms *Cyclotella* and *Melosira*. One of these was by Mr. Jabez Hogg, the other—more recent—by Mr. Minchin. They both assert that the discovery of these so-called pseudopodia is a very old one; that they have often been described; and Mr. Hogg gives their compositions. I should be glad to make two remarks in reply.

Firstly, my slides have been examined by a majority of the best Diatomists and Biologists in England, and not a single one has suggested that he had either seen or heard of anything like these pseudopodia. They have also been sent to

several of the best foreign Diatomists, who likewise failed to refer me to any previous description. Mr. Hogg and Mr. Minchin dealt with generalities. I should be glad to have a reference to any paper where these pseudopodia have been described.

Secondly, I have found these diatoms with the pseudopodia in almost every bit of water—whether pond or stream—in the South of England, where I have looked for them. They occur in many places in London and the suburbs, and from Yarmouth to Sheerness, Eastbourne and Wiltshire.

I advise my critics to take the trouble to get some of these diatoms and study the phenomena for themselves. If they will do so, I do not think we shall hear much more about either opinions, processes, or old discoveries.

J. G. Grenfell.

[I am much surprised to find Mr. Grenfell including me among his "critics," with regard to the pseudopodia of diatoms. I do not profess in any way to be an authority on the subject, and the criticism which Mr. Grenfell kindly attributes to me was quoted word for word from Mr. Jabez Hogg. It was Mr. Hogg, not I, who asserted that the discovery of these so-called pseudopodia was a very old one, and that they have often been described. Mr. Grenfell seems to have overlooked the inverted commas in which these statements are placed. My only object was to place before English readers an account of Professor Bütschli's researches upon the movement of diatoms, and to point out that "any form of pseudopodia is quite inadequate and unnecessary to explain" the phenomena observed by him, without, however, expressing any opinion of my own as to the existence or nature of these so-called pseudopodia.

E. A. Minchin.]

Palæontographical Society.

The attention of the Council of the Palæontographical Society having been called to a paragraph in your January number, in which you wrote, "In the case of several of these monographs, announced as in preparation, we believe the statement is false, and that members of the Council are aware of the fact," I am directed to state that all these monographs have been promised, and that the promises have not been withdrawn. Thos. Wiltshire, Secretary.

[Promises are fickle. We have already proved the accuracy of our assertion in the case of two announcements, and the Secretary might be profitably "directed" to make further enquiries. The continued official repetition of promises, which are well known to be valueless, prevents other palæontologists from undertaking works that are much wanted.—Ed.]

TO CORRESPONDENTS.

N.B.—CHANGE OF ADDRESS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., 29 *Bedford Street, Strand, London, W.C.*

All Advertisements *to be forwarded to the sole agents,* John Haddon & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

H. Ullyett (Folkestone).—The wingless bird referred to is not a species of *Apteryx*, but the well-known Weka Rail (*Ocydromus australis*). Examples of this common New Zealand Bird can generally be seen in the aviaries at the Zoological Gardens in London.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 13. Vol. II. MARCH, 1893.

NOTES AND COMMENTS.

Natural Science at Oxford.

REMUNERATIVE positions affording facilities for scientific research in Britain are so few, that all who are interested in progress will sympathise with every effort to retain, for purely scientific work, the services of those who have shown an aptitude for original investigation during their University career. There must be Fellowships and Lectureships in the colleges to enable the graduate choosing science as a profession to pursue his work during the interval between the taking of his degree and a possible permanent appointment; and the more of these endowments there are, the greater is the inducement for young University men to aim at something higher than mere honours in an examination. In the interests of Natural Science, we thus desire to call attention to "An Appeal to the Governing Bodies of the Colleges within the University of Oxford," now being circulated by the Linacre Professor of Comparative Anatomy, Dr. Ray Lankester. Though primarily a matter for the University itself, the issues involved have so direct a bearing on the future progress of the biological and geological sciences in this country, that the "Appeal" demands the support of all investigators. The University of Oxford is second to none in the repute of its Professors and Lecturers in Natural Science; its laboratories, museums, and scientific libraries are among the finest; but, if the governing bodies of the colleges continue practically to ignore the School of Natural Science, all these provisions and advantages are in vain. According to Professor Lankester's statement, the graduates in Science *are* ignored to an almost incredible degree, and hence the necessity for a vigorous protest.

It appears that the total number of open Fellowships in the Oxford colleges is at present about 300, and that of these only thirty

are held by graduates devoted to scientific research. Nearly all these, too, are in the hands of physicists and mathematicians; while only one is held by a zoologist. Considering that more than 200 classical and historical scholars are thus favoured, the division of honours is certainly as unfair an arrangement as can well be imagined. We may appropriately ask, with a critic in the *Daily Chronicle* of February 13, whether the amount of research published by Oxford men in classics and history is 200 times as great as that accomplished in zoology; and, with that writer, we may further truly exclaim:—

"The reverse might not be quite accurate, but it would be most unquestionably nearer the truth. A great mass of Oxford fellows are persons who batten for life on the college funds, because, when boys, they passed a successful examination. Like the Rev. Mr. Pontifex, they obtained honours in examination. They, furthermore, resemble that divine in having afterwards abandoned their studies. At twenty-three years of age it is extremely creditable to have got a 'first-class,' but the reputation so earned can hardly be considered a claim at sixty-three. I have heard it urged in palliation of the inactivity of the classical fellow, as contrasted with the science man, that there is less new work to be done in the former subjects. I beg leave to doubt this; but if it be true, then all the more reason to encourage a subject where there is still a wide field open to the investigator. If the colleges were to bestow their fellowships not by examination, but after the candidates have established some claim by original investigation, there would be a wholesome weeding out of those whose interest in their subject and capabilities were of the purely text-book order. With a test of this kind there would be some hope for the adequate recognition of science, and really able men would be attracted to Oxford, and stay there. Above all, that most delusive weed of university growth, the man of promise, would be rooted out, and his place taken by the man of performance."

As Professor Lankester remarks in his "Appeal," there is not much doubt that financial considerations on the part of those Fellows who are interested in the subjects taught in a college are at the root of the difficulty. Until some change can be effected in these fundamental matters, there is thus but slight hope of a due recognition of Natural Science, which can only be taught in the University laboratories and museums. That the reformation will come sooner or later is inevitable, and we trust its advent may be speedy.

Mechanical Evolution.

We have on previous occasions alluded to the speculations of the American school of zoology in reference to the mechanical origin of the various arrangements observed in the vertebrate skeleton. According to these authors, under the leadership of Professor Cope, there is every reason to believe (i) that living bony tissue is plastic, and therefore readily modified in its form by impacts, strains, friction, &c., and (ii) that acquired characters are inherited. Every process

in the evolution of part of a vertebrate skeleton is thus to be explained on purely mechanical principles.

To the discussion of this subject another interesting contribution has just been made by Professor Cope, who describes two cases of a repaired elbow-joint, one in a man the other in a horse (*Proc. Amer. Phil. Soc.*, vol. xxx., pp. 285–290, pl. ix.). In his well-known work on "Animal Mechanism," Marey has already described instances of the natural healing and repair of diseased or dislocated joints—how a new articulation is often completely formed, not only with accurately fitting faces, but also provided with the requisite ligaments and synovial fluid. Professor Cope's paper thus deals merely with the details of two remarkable examples of this repair, in which a new elbow-joint was completely formed after a dislocation. As the author remarks, if the mechanical necessities of a case of this kind lead to the production of a completely new and finished joint within the space of a few months, "how much more easy has it been for stimuli of allied character to develop the features of normal articulations during the ages of geologic time."

"We have here, also," says Professor Cope, "an instructive lesson as to the matter of inheritance. Everyone knows that mutilations, luxations, &c., are not usually inherited. This is because they are not 'acquired,' in the proper sense of the word. Since characters, truly acquired, are inherited, it is evident that a long continuance of the stimulating cause is necessary to produce a true acquisition. The difference between a character produced by causes apart from the normal life of an animal, and not repeated, and those produced by causes operating daily and hourly for geologic ages, is necessarily very great."

A Glimpse of the Tropics.

In the recent December and January numbers of the *Botanical Gazette*, Mr. D. H. Campbell gives an account of a six weeks' vacation spent last summer in the Hawaiian Islands. In position, these islands are unique, being "more isolated than any other land of equal area upon the globe." They are 2,350 miles from the nearest part of the American mainland, the bay of San Francisco, and about the same distance from the Marquesas and Samoa Islands to the south, and the Aleutian Islands a little west of north. As Wallace remarks in his *Island Life*, they are "wonderfully isolated in mid-ocean," the nearest of the widely-scattered coral reefs and atolls being six or seven hundred miles distant, and all nearly destitute of animal or plant life.

The group consists of seven large inhabited islands and a few rocky islets, extending in a S.E. direction just within the tropic of Cancer. Hawaii, by far the largest and most southerly, is 70 miles across and about the size and shape of Devonshire; the greater part

is, however, rendered sterile and uninhabitable by its active volcano and the lava-deposits. The entire archipelago is volcanic, and separated from the nearest continents by great depths, so the islands must have been always as isolated as at present.

The flora was well worked up by Hillebrand, and, as might be expected, is very peculiar; of 800 indigenous species of seed plants and ferns, 653 or 75 per cent. are endemic; of seed plants alone, 81 per cent.; and of dicotyledons, 85.

The capital, Honolulu, which is situated on Oahu, one of the more northern islands, "is like one great botanical garden." This is said to be largely due to Dr. Hillebrand, who introduced many foreign plants, and his place, kept much as it was when he left the islands, "was a very remarkable collection of useful and ornamental plants from the warm regions of almost the whole globe." Very striking to the traveller from temperate climes is the great variety and number of palms; the beautiful royal palm (*Oreodoxa regia*), with its smooth columnar trunk and its plume-like crown of leaves, betel-nut palms, (*Areca*), the wine palm (*Caryota*), the sugar palm (*Arenga*), and many fan-palms. The young coco palms are beautiful enough, though, unfortunately, very subject to the attacks of an insect which eats the leaves, but in old specimens the trunk is too tall for its girth, so that the trees look top-heavy. The great preponderance of Leguminosæ, especially the sub-orders Cæsalpinieæ and Mimoseæ, is also remarkable. All about the town is the rapidly-growing algaroba (*Prosopis juliflora*) a graceful tree, with fine bipinnate leaves and sweetish yellow pods. The pods are largely used for fodder, and the wood forms the principal fuel-supply for Honolulu. The monkey-pod (*Pithecolobium Samang*), tamarind, and various species of *Bauhinia* and *Cathartocarpus* were also noticed, besides a great number of shrubs and trees, with showy flowers or leaves, mostly familiar from pictures or the greenhouse; such were several species of *Musa*, the traveller's tree (*Ravenala madagascariensis*), and the beautiful *Hibiscus Rosa-Sinensis*. At Puna Hou College is a hedge of night-blooming cereus 500 feet long. Of fruit trees the mango, bread-fruit, and guava are common, also the alligator pear (*Persea gratissima*) and the papaya.

Away from the city the luxuriant vegetation is strange. Along the sea-shore the plain is almost destitute of trees, save for an occasional coco palm, while in the fertile lowlands near the sea are the principal cane- and rice-fields.

The valleys at the back of the city, though very rainy, richly repaid a visit by the luxuriance and variety of their vegetation. Grass-covered hills give way as one proceeds to thickets of *Canna* and a rosy-white *Clerodendron*, while the curious screw-pine is occasionally seen, though much more abundant in some of the other islands. Several showy ipomæas are very common.

With increase of moisture, masses of ferns increase in beauty and number, and at about 1,000 ft. elevation species of *Cibotium*

appear, the genus containing the largest tree-ferns of the islands. Here, too, is seen the tiny *Trichomanes pusillum*, forming dense mats on rocks and tree trunks, and looking like a delicate moss; the full-grown frond is not more than half-an-inch high. Epiphytic ferns also become frequent, species of *Acrostichum*, *Polypodium*, and the beautiful bird's-nest fern (*Asplenium nidus*). Everywhere are thickets of *Freycinetia*, very troublesome to get through.

Of the trees of this lower forest region, by far the most conspicuous is the euphorbiaceous *Aleurites moluccana*, with pale silvery-green foliage; and a little higher up the phyllode-bearing Koa (*Acacia Koa*), the principal timber tree, with wood not unlike mahogany in appearance. Conspicuous also is the mountain apple (*Eugenia malaccensis*), with beautiful crimson fruits, and, higher still, the nearly-related *Metrosideros* forms a striking object with its grey-green leaves and scarlet, feathery flowers.

Hawaii is reached by steamer in about thirty-six hours from Honolulu. On the way are passed Molokai, the barren and forbidding leper settlement, Lauai, and Maui, the next largest island.

Hawaii consists of three great volcanic cones, only one of which is now active. Mahukona, on the leeward side of the island, the first landing-place, showed a forlorn expanse of bare lava, with scarce a trace of vegetation, quite a contrast to the next stopping-place, Hilo, where the luxuriant vegetation comes down to the water's edge.

The forest here is most interesting, and owing to the great annual rainfall (180 inches) and its more southerly position, like the rest of the flora, more tropical in character. Ferns and mosses luxuriate, while flowers are almost entirely wanting. Many tree-ferns, species of *Cibotium*, had trunks from 15 to 20 or even 30 feet high, with fronds 18 to 20 feet long. Growing on their trunks were epiphytic ferns, the peculiar *Ophioglossum pendulinum*, with its long, strap-shaped leaves, and exquisite species of *Hymenophyllum* and *Trichomanes*. Of the terrestrial ferns, the tropical *Gleichenia dichotoma* and *Marattia Douglasi* were noticeable, and several species of *Lycopodium* and *Selaginella* were common.

Coffee is extensively planted in this region as well as on the lee side of the island, and the quality of the berry is exceptionally fine.

Near the volcano, Kilauea, about 4,000 feet above sea-level, are many interesting plants, one a *Vaccinium*, with berries resembling cranberries.

The leeward side of the island is dry and vegetation scanty. The soil, however, is very fertile, and, when water can be had, produces magnificent crops of tropical products, such as pine-apples, coffee, sugar, &c.

A flying trip was made to Kauai, the oldest, geologically, and the richest, botanically, of all the islands. Hillebrand states that not only is the number of species larger, but they are more specialised than in the other islands. Here were seen several woody Lobeliaceæ.

As in all the islands, there is a great difference between windward and leeward sides, the former affording some of the most beautiful scenery and luxuriant vegetation of any seen during the trip.

KIDNEY TUBES IN AMPHIOXUS.

DR. THEODOR BOVERI, of Munich, has made one of the most striking zoological discoveries of recent times. In Spengel's *Zool. Jahrbücher* (vol. v., p. 429), he describes and figures a system of segmental canals in *Amphioxus*. These occur all along the region of the pharynx—one pair for each two pairs of gill-slits. They lead from the body-cavity to the atrial sac. There are 90 pairs altogether: the first and the last open by single ciliated funnels into the coelome; the others by from five to seven funnels. A network of blood-vessels covers the funnels, and Boveri shows that carmine and indigo-carmine taken into the blood reach this network and are removed by the funnels. He gives a series of striking arguments in favour of regarding half the atrial cavity as the homologue of the primitive pronephric duct of Craniata. The whole paper is wonderfully convincing; and one has only to look for a moment at sections of *Amphioxus* to see the canals, and to be astonished that one has not seen them before.

To readers who are not specialists in Zoology, the discovery of kidney tubes in Amphioxus may seem a matter of little moment; but the presence of a definite order and sequence in the development of the excretory organs is one of the strongest links binding together the Chordata—the group of animals which includes the vertebrates and a few lowly forms which have only the gelatinous rod which, in vertebrates, is the embryonic predecessor of the vertebral column. In all the members of this group, the excretory organs first appear as simple tubules, like the tubules of worms. These open at one end by a funnel into the body-cavity; at the other, into a longitudinal duct at each side. In higher vertebrates, like birds and mammals, a complicated series of changes supervenes, and the original excretory organ comes into use as part of the reproductive apparatus while the permanent kidney is developed in connection with the posterior end of the original organ. Probably there is no plainer instance of the embryological law that the individual in its development travels along the path its ancestors traversed in their evolution; for practically every term in the series of changes in the higher vertebrates is preserved as the adult condition in some lower form. It was a puzzle why Amphioxus, which in so many of its organs preserves stages embryonic in higher animals, should be inexplicable in the set of organs most clearly identical in other chordates; and now Boveri has shown that its excretory organs are precisely what analogy and homology alike demand.

More Notes on Seedlings.

The January Bulletin of the Torrey Botanical Club opens with some "Studies upon Akenes and Seedlings of Plants of the Order Compositæ," by W. W. Rowlee, illustrated by five plates. The account, which occupies 17 pages, is much less comprehensive than that given in Lubbock's recent work (vol. ii., pp. 98–161), and the general observations are of less value, being based on fewer experiments. Thus the cotyledons, we are told, vary in shape from spathulate to orbicular, but Lubbock gives several instances of linear cotyledons,—*Bidens humilis*, species of *Ursinia*, and *Coreopsis gigantea*; the American author describes one species of *Coreopsis* (*C. discoidea*), which, however, like another species (*C. laciniata*) included by Lubbock, has broader cotyledons. Nor is any mention made of the interesting polymorphy of achenes, which obtains in the Old World genus *Calendula*. Mr. Rowlee's studies form, however, a valuable addition to our knowledge, as the greater number of his species are not included in the larger work, and the genera are in many cases unrepresented. Moreover, figures are given of every species described, both of the fruit and seedling, and often two or more stages of the latter.

The cotyledons of the Ox-eye Daisy are oblong, like those of the Corn Marigold and another species of *Chrysanthemum* (*C. carinatum*) figured by Lubbock, but Rowlee makes the two following leaves spathulate and entire, thus differing widely from their deeply-toothed character in the other two species. The seedling of the Yarrow corresponds with the description in the English work, except that there is no mention of any hairiness on the first leaves; it would be interesting to know whether they are glabrous in the New World, or the hairs have been merely overlooked. One *Artemisia* is described, viz., Wormwood (*A. Absinthium*), and its seedlings, we are told, may be distinguished from those of the Yarrow by the oblong shape and entire margin of the second and third leaves after the cotyledons. Comparison with the larger treatise shows that this is not a generic distinction, for there we find that in *A. annua* the corresponding leaves are three-toothed as in the Yarrow though in another species (*A. Mutellina*) entire.

The Helianthus (*H. divaricatus*) mentioned bears a general resemblance to *H. cucumerifolius*, included by Lubbock, the cotyledons being ovate and obovate respectively, while in each case the first internode and following leaves are somewhat densely hairy.

In speaking of the embryo, cotyledons and hypocotyl are invariably distinguished, the latter term including everything below the cotyledons, and consequently the radicle. This seems a pity; the radicle is an organ so well marked, not only from a morphological, but also from physiological and anatomical points of view, that it is a pity to ignore or slight its presence in the earliest stages.

Some remarks are also made on the germination of the seeds,

That of annuals and biennials is much prompter than of perennials, while a much larger percentage of the former germinate, giving larger and hardier seedlings.

The percentage of germination was found to be greatest in "persistent field weeds," only two of which, however, were studied; the Burdock and Dandelion; that of wayside and fence-row weeds like Elecampane (*Inula Helenium*) comes next, while in those which cannot be characterised as weeds it is considerably less, the general averages being 49, 33, and 8 respectively. This is taken as direct evidence that the vitality of seeds of a species is a factor in determining its abundance and ability to become a weed. Twenty-five seeds of each species studied were sown with the upper (pappus) end downwards, and a similar number erect; 149 of the former germinated, and 178 of the latter. The radicle emerges from the lower end, and these numbers show that it is a disadvantage for this end to be uppermost, a position, moreover, which the fruit would not naturally take, since the pappus, acting like the feathers on an arrow, tend to keep it erect.

The Hidden Coal-fields in the South of England.

Geologists who are interested in the study of the hidden coal-fields in the South of England will find some exceedingly suggestive remarks in two papers published by M. Marcel Bertrand. The earlier one, though entitled "Sur la Continuité du Phénomène de Plissement dans le Bassin de Paris" (*Bull. Soc. géol. de France*, ser. 3, vol. xx., p. 118), relates largely to the anticlinal and synclinal folds which cross the English Channel and North Sea. We are unable, however, altogether to follow the author when he accepts the present irregularities of the sea-bottom as corresponding to ancient undulations in the strata beneath. It is so rare in our seas to find an undisturbed rocky bottom, except near the Straits of Dover, and shifting sand-banks are so common, that it seems more reasonable to suppose that the irregularities of the sea-bed are mainly due to the scour of the tide. Yet some of these irregularities are undoubtedly ancient hills and valleys, or lines of escarpment.

M. Bertrand's second paper, "Sur le Raccordement des Bassins Houillers du Nord de la France et du Sud de l'Angleterre" (*Ann. des Mines*, Jan., 1893), deals mainly with the hidden coal-fields, their probable position, and their extent. The maps published with these papers are excellent, and ought to throw a great deal of light on the trend of the Coal-measures in our southern counties.

We would also direct attention to Mr. Brady's report on the Dover Coal Boring, reviewed elsewhere.

Prehistoric Archæology.

The "Congrès International d'Archéologie Préhistorique," which met last year at Moscow, has led to the publication of an in-

teresting series of papers on the Quaternary Geology of Russia. The most valuable of these, especially to the foreign reader, will be Professor S. Nikitin's outline of the present state of our knowledge of the subject. In this summary it is pointed out that Palæolithic man existed in Russia contemporaneously with the mammoth during the second half of the Glacial Epoch, but only towards the southern limit of the glaciated area. In the more northern districts all the implements yet found belong to the more advanced Neolithic races. The existence of a distinct Interglacial Period, like that recognised by Scandinavian and German geologists, has not yet been satisfactorily demonstrated in Russia.

It is impossible to criticise this paper without an intimate knowledge of Russian geology, but it may be worth while to remark that in Western Europe also, the relics of Palæolithic man are only found in regions beyond the southern limits of the last glaciation. Is it possible that in Russia, as was perhaps the case in Britain, Palæolithic man was Interglacial? His relics are not found in the areas covered by ice during the last glaciation, and this has been explained as the direct result of the advance of the ice, which ploughed up and destroyed all the pre-existing Palæolithic deposits as far as it could reach.

Our Monthly Selection.

Mr. James Payn has somewhere expressed regret at the supposed fact that men of science are unable to make their subject popularly interesting. It is certainly true that "popular science" does not, as a rule, result from the literary activity of persons qualified to write upon such subjects. We have had occasion more than once to point out that this, if a blessing, is one securely wrapped up; for it is to be presumed that an individual, if he cares to read scientific articles, prefers them to be fairly reliable, else why read an article which is clearly meant to be instructive?

The editor of a recently-started journal, entitled "The Sketch," advertises his willingness to consider paragraphs which are "smartly" written. As an indication of what he wants, attention may be directed to a paragraph in the first number—"Why not a Professor of the Zoo?" The writer talks a little about the parietal eye; for this overworked organ of vision appears to have just filtered down through the "dailies" to the "weeklies." In the course of time it may perhaps reach the monthlies. We present Mr. Payn gratis with the suggestion. The antiquity of the points with which it deals is, however, not the only claim which this paragraph has to be considered smart. Many of us will be astonished to hear that the "hole" in a baby's head at birth is the vestige of a parietal eye! The writer concludes with a lament that there is no professor at the Zoo to make men acquainted with these facts (!). Even though he presumably possesses no parietal eye himself, the two usual organs of vision would

be enough for him to observe that every year for the last—we don't know how many, there have been popular lectures on Zoology given at the Gardens. For the last few years they have been delivered by Mr. Beddard; and it is clear that a course of lectures on Zoology would not come by any means amiss to this paragraphist.

WE are always glad to note the gradual percolation of science through the lower strata of our population, and we have derived much instruction from a recently-issued pamphlet entitled "Leaves from the Book of Nature, or Stepping Stones in Creation," by L. Piers. The author seems to be a great admirer of the Geological Department of the Natural History Museum, and his acquaintance with the collection, if not extensive, is at least peculiar. He traces the history of our globe from the "Archaen" rocks "composed of schistose or granitic character," through the Cambrian period when the sea was at 90°, the Silurian and Devonian, with their Graptolites, Ammonites, Crinoids, and Nummulites, the Carboniferous, "Treassic" and Jurassic, then "the Cretaceous system composed of white chalk," down to the Tertiary periods and the great ice age. "Wonderful and marvellous truly are the mysteries of Nature"! thus we read of that "curious crustacean called *trilobite*; they were in three great families, *olenellus*, *paradoxides*, and *olenus*," then the Crinoids, whose "province was to check the too great increase of certain other creatures, while in their turn they were devoured by the larger fishes" [poor fishes!]. But, oh! the "strange and long reptile, named *Deinosaura*," "the *Plesiosaurus*, the earliest crocodile," the "extraordinary *Petrodactylus* and *Trinoceros*."

Two notes on page 16 inform us, first, that "the vertebrate kingdom are in five great divisions: fish, reptiles, birds, mammalia, man;" secondly, that "The visitors will find at the entrance to the Geological Department some excellent illustrated catalogues." Are these catalogues really so bad? or did Mr. Piers write his book before he read them?

IT is well-known that a long-haired variety of the tiger ranges at the present day as far north as the region of the Amur, on the frontier of China and Siberia. The animal is thus capable of living in a cold climate, but the astonishing discovery has just been made of evidence of its former range to a northern latitude even within the Arctic Circle. Among the fossil bones collected in the New Siberian Islands and on the adjoining mainland by a Russian expedition despatched from St. Petersburg in 1885, there are five characteristic limb-bones of the tiger. They are described by Dr. J. D. Tscherski in his report on the collection (*Mém. Acad. Imp. Sci. St. Pétersbourg*, vol. xl., no. 1, with 6 plates), and occurred in the same deposits as bones of the musk ox, mammoth, &c.

In the January number of the *Quarterly Journal of Microscopical Science* (vol. xxxiv., p. 317), Mr. Arther Willey has another interesting study on the ancestors of the Chordates. As the result of a series of observations, he groups together *Cephalodiscus* and *Balanoglossus* as Protochordata with mouth ventral, and with no endostyle; Ascidians and Amphioxus, as having an endostyle and a dorsally-situated mouth. In these two groups, *Cephalodiscus* is parallel with the Ascidians. Both are sessile, possess a U-shaped alimentary canal, have a small number of gill-slits (one and three respectively), and reproduce by buds. *Balanoglossus* and *Amphioxus* are also parallel, being free-swimming, with straight alimentary canal, many gill-slits, and without the power of budding. That difficult form, *Appendicularia*, which was long regarded as the most primitive of the Ascidians, Willey considers as a reduced and secondary derivative from the Ascidian stock.

The application of photography to the study of animal locomotion is still being extended. M. Marey, we are glad to record, is continuing his observations on the swimming of fishes, and has just published the result of photographing a skate (*Comptes Rendus*, vol. cxvi., pp. 77–81, Jan. 16, 1893). The body of the fish was firmly fixed, and the motion of the great pectoral fins was photographed from the side and from the front. The undulation of the fins proves to begin at the anterior border, and the whole progress of the motion bears a singular resemblance to the movements of a bird's wing during flight. M. Marey proposes now to devise means of studying the locomotion of the skate under natural circumstances, and will attempt to determine the extent and directions of the currents produced in the water.

There are many strange phenomena connected with the life of protozoa within animal tissues, but a recently-announced discovery by Professor W. A. Haswell (*Proc. Linn. Soc. N.S. Wales*, ser. 2, vol. vii., 1892, pp. 197–199) is, perhaps, the most remarkable of its kind. It is well-known that certain amœboid forms spend the whole of their existence within a single cell of the tissue of their host, and it is the ordinary rule among gregarines to begin life within a cell; but it now appears that even certain freely-swimming flagellate protozoa are capable of passing through life in a similarly restricted sphere. On examining a turbellarian worm from a pond in the neighbourhood of Sydney, Professor Haswell found one or more minute green flagellate protozoa in many of the cells. These were observed to be in an active state, and appeared to be closely related to the familiar *Euglena*.

Dr. Axel Goës has just described, in the *Bull. Mus. Comp. Zool. Harvard Coll.* (vol. xxiii., no. 5), the remarkable new form of arenaceous Foraminifer found during the dredging expedition of the U.S. Fish Commission steamer "Albatross." This form, of which the finest specimen measures 190 mm. in breadth, is by far the largest Foraminifer known. It consists of a "strong network of bundles of very fine chitinous threads, measuring in thickness 0·003–0·006 mm., incorporated with a thin layer of finest sand and *débris* of shells. . . . The test is leaf-formed, with outlines usually describing a triangular, fan-like or reniform figure, with more or less strongly arcuated edge, the whole reminding one of a Padina alga of 0·5 to 2 mm. in thickness." In its earlier stages, this Foraminifer has the fan-like shape of *Pavonina*, but, as the chambers increase in number, they no longer arch over the centre, but terminate with a blunt end at the top, the lower end being produced with long root-like appendages, which "serve probably as fastenings to the botton, where they often are entangled in masses of a Rhizammina." Dr. Goës compares this form with the *Jullienella* of M. Schlumberger (*Mém. Soc. Zool. France*, iii. (1890), p. 211). Dr. R. Hanitsch, on the other hand, writes to *Nature* (Feb. 16) to say that he thinks it is one of the deep-sea Keratosa, and suggests that it may possibly be referred to *Stannophyllum zonarium*, Haeckel.

We understand that advices have been received from the Villiers Expedition to Lake Rudolph, dated Vitu, December 20, 1892. Lieut. Villiers has had fever, and the command has fallen on Lieut. Stanford. Mr. J. W. Gregory is reported to have been in excellent health and spirits, and the expedition was to be on the march in a few days. Subsequent statements, however, have appeared, which show that Lieut. Villiers has recovered, and has joined the Portal expedition to Uganda.

We are glad to observe that, since our last issue, Professor Lapworth has closed the controversy on the geology of the Scottish Highlands, by contributing to the *Daily Chronicle* of February 8 a concise historical statement which we can endorse.

I.

The Nucleus in some Unicellular Organisms.

THE importance of continuous and exhaustive studies concerning the ultimate and irreducible nature, morphology, and function of animal and vegetable cells is universally seen. The cellular elements of tissues began to be observed even by Hooke two centuries and a quarter ago; and some further advances were made, but it was not until 1831 that Robert Brown, the great pioneer in botany, took the first great step leading to a practical advancement of the subject. He gave definite knowledge of vegetable cells, and he demonstrated that the nucleus was one of its normal elements.

It need hardly be remarked that the cell-theory proper had its foundation in the work of Schleiden, but by him it was not extended beyond the structure of plants; he clearly defined the vegetable cell as the elementary organ which constitutes the sole essential form-element of all plants, and without which a plant cannot exist; and as consisting, when fully developed, "of a cell-wall composed of cellulose, lined with a semi-fluid nitrogenous coating."

The cell was thus to Schleiden a vesicle with semi-fluid contents.

In the year following (1839) Schwann showed that the Animal Kingdom was ultimately as cellular in structure as the Vegetable; but to the vesicular wall and the semi-fluid lining of Schleiden he added a third element, the *nucleus*, and he deemed this essential to the history of the cell, at least in some period of its life.

From this time, the triple elements of the cell were accepted as its normal condition; but investigation made the continuance of this belief more and more uncertain. It was shown that cells multiplied by "budding," and that the nucleus underwent fission when the cell divided; and also that no cell could take origin save from a parent cell.

Soon it was demonstrated that there was no vital importance attachable to the cell-wall; and in 1857 Leydig declared it entirely unessential, and defined the cell as a "soft substance enclosing a nucleus."

Subsequently, Max Schultze contended that the life of a cell might be complete without a nucleus, but the cell was held to be the ultimate morphological unit in which life was manifested. Every

living thing presented itself as a sum of vital unities, any one of which manifested the properties of life.

The living matter of the cell was held to be *sui generis*. Not-living matter could not, by any combination we knew of, apart from the genetic intervention of living things, take on its unique properties. Only from the living could the living arise. This living matter was, and is, called protoplasm; and even now a cell can hardly be more clearly defined than as a small mass of protoplasm enclosing a peculiarly organised element known as the nucleus.

The further study of cells, aided by the constantly increasing power and precision of our microscopes, revealed complexity, reticulation, striation, foliation, radiation, and other appearances, at present undetermined by the best observers with the finest accessible optical powers; while the nucleus is seen to be highly complex, and subject to the most striking internal modifications during its cyclic changes.

To pursue or even indicate the history of the prolonged and untiring work upon the cell during recent years is no part of the object of this paper; enough to recall that the nucleus is found to be the initiating centre of all the great changes which the cell undergoes.

But the study of the cell has been conducted in the main (1) upon cells belonging to highly complex organisms, and, therefore, as vital unities of that organism must partake, in all their mutations, of the complexities involved in the developmental organism of which they are units; and (2) the different processes of cellular change and nuclear modification have been studied, of necessity, after death, and under the influence of desiccation and staining.

For years it has impressed itself upon the mind that the great problem of the nature and behaviour of the cell should at least be begun where the problems to be solved ought to present themselves in their simplest condition, and that would appear to be amongst the unicellular organisms—the organisms whose complete life, when traced through all its cyclic changes, begins in a cell and ends in a cell, presenting an elementary though, in their case, a permanent condition in the evolutional history of more complex organic forms.

These are easily and everywhere accessible in that remarkable but minute group of organisms known as *Saprophytes*. They succeed the saprophytic Bacteria in the destructive fermentation of large masses of dead organic tissue. There are only a few of these forms known, and their successive action upon the decomposing mass is both mechanical and physiological.

They are strictly unicellular; the majority are nucleated cells; but the problem of the cell is rendered the more interesting by the study of them, from the fact that amongst the group are distinctly non-nucleated forms.[1]

[1] *Vide* the author's researches, *Month. Micro. Journ.*, vols. x., xi., xii., xiii., xiv., xvi.; also *Proc. Roy. Soc.*, 1878, and *Journ. Roy. Micro. Soc.*, ser. ii., vols. v. and vi.

As typically represented, they are oval or suboval, are possessed of one or more flagella, and have an average long diameter of about the 1-4000th of an inch.

Two fair representatives of the group as re-examined during the last four years with the advantage of our immensely-improved object-glasses and eye-pieces are represented in Fig. 1. A represents a form about the 1-4000th of an inch in long diameter, and B a kindred form the 1-5000th of an inch measured in the same way. They are in the drawing magnified 1,800 diams.

A is distinctly nucleated, and trails a flagellum when swimming, but (as in the drawing) often "anchors" it to the "floor" of the stage; and from this position acts by springs upon the masses of decomposing matter around it. The form B simply swims either forward or backward by means of its flagellum at either end.

The life-cycle of A may be taken as representative, and will show what was actually known by us before the investigations upon the nucleus made within the last four years with our present apochromatic microscopic illumination and apochromatic objectives.

Taking A, Fig. 1, as the normal form, in certain conditions of the organism now definitely known to us, a sudden opening of the "beak" to which the flagellum is attached (*a*, Fig. 2, C) would arise partly dividing the flagellum, as shown in the figure. While this was increasing, so far as we could discover in our earlier studies—in the course of a couple of minutes—the nucleus C showed a definite dividing line, and the "trailing flagellum" also rapidly split, as at *b*.

The division first initiated widened (D, Fig. 2, *e*), and the front flagellum became wholly divided, the trailing one not splitting so fast; but the nucleus showed, as at *d*, a strong tendency to fission.

Very rapidly the bisection ensued, until generally, in not more than four minutes, the division had been wholly effected lengthwise, as in Fig. 3, E, *g*, *h*, and the nucleus was wholly divided save for a fine thread connecting the parts, as at *i*, K, and the trailing flagellum was nearly divided.

Soon the protoplasmic neck connecting the dividing forms was almost wholly gone, as in Fig. 4, F, and the two nuclei, *n*, *o*, were as perfect as in Fig. 1, A, that is to say, a highly refractive body with a distinct envelope, or border; and the two perfect forms, *l*, *m*, Fig. 4, become disconnected, dividing the trailing flagellum to its root, and each being free.

This process is exhaustive in the great majority of the forms. By following successive fissions—that is, one-half of each division—it was found that the end came at from four to five hours. In other words, this process of division ceased by the death of the last-divided form. But this only applied to about eighty-five in every hundred. The remaining fifteen go through a very curious change.

Fig. 5 represents a final form derived by fission. Its chief distinction is a strong nucleus, and always a free-swimming condition.

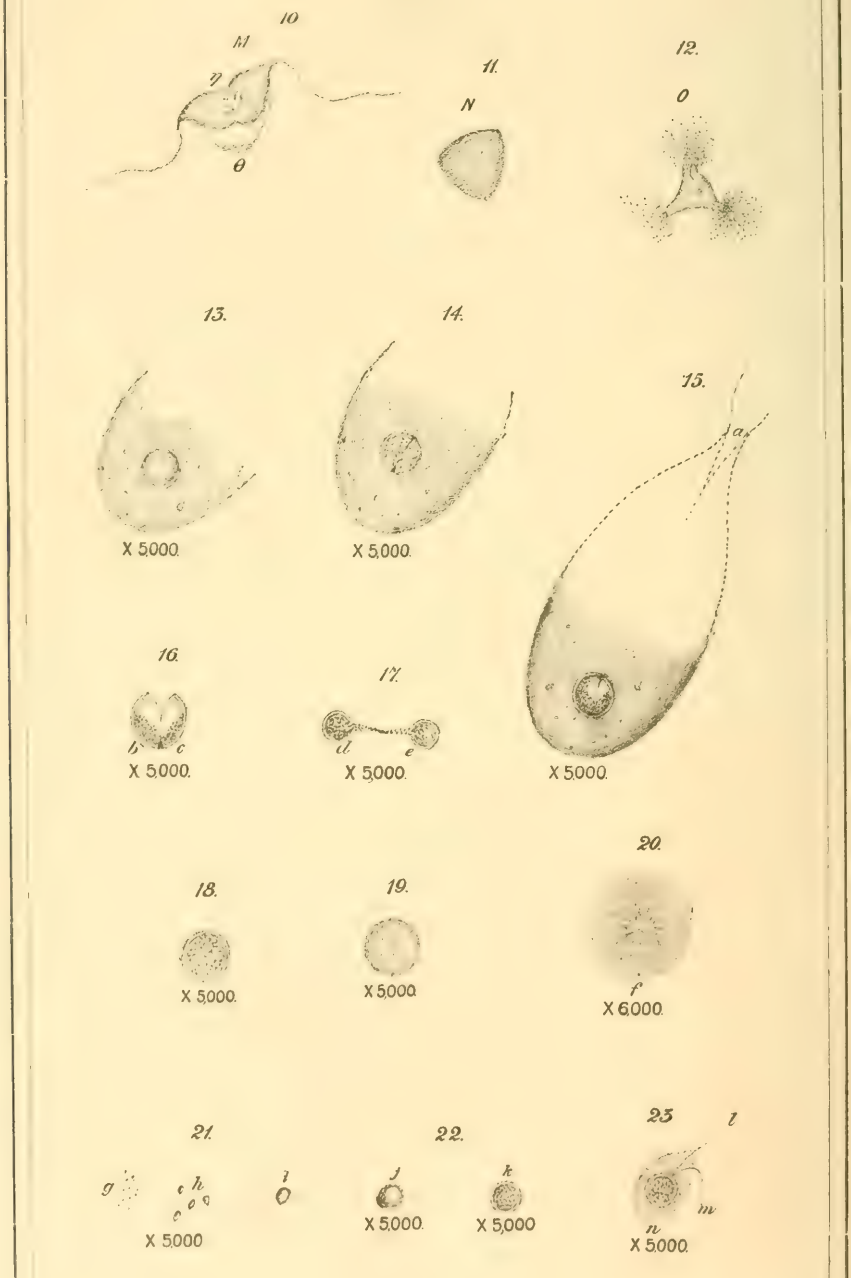
In the course of from five to seven minutes there would supervene an amoeboid condition, not striking, but still visible, as shown in H, Fig. 6, where the body-substance would oscillate from the hollow condition shown to the lateral expansions indicated by the dotted lines at *s*, *t*, a vacuole—curiously like an "eye-spot"—would appear, exhibiting very marked opening and shutting, as at *v*; and the nucleus, *u*, was driven to the middle of the body, greatly enlarged, and, losing its nucleus-like aspect, was torn apart in irregular halves; meanwhile, a very strong sigmoid division-line, shown in J, Fig. 7, at *w*, *x*, became extremely marked, and the divisions of the nucleus were pushed one on each side of this line, as at *w* and *x*. At this time a point of protoplasmic material was pushed out, as at *y*, dividing into two fine threads.

A gliding motion now ensued, by which *w* glided off from *x*; this is seen at Fig. 8, κ, α, β, showing two normal forms nearly freed from each other in this way; but in our earliest work it was quite manifest that the nuclei of these two forms had quite strikingly changed, having undergone some radical alteration, and trailing flagella were formed at *z*, partly by growth and partly by division.

Soon after total freedom from each other, these modified forms swam freely for some minutes, and then directed their motion to some part of the field where the ordinary forms (with the usual nuclei) were in greatest abundance, and almost immediately what was seen to be an act of fusion commenced. The initial stage is shown in Fig. 9, L, where the unlikeness of the nuclei is seen at γ, δ.

The fusion takes place rapidly up to a certain point, for in eight minutes a large part of the body-substance of each has fused with the other, as seen in 10, M, where the nuclei are fully united, and a radial or star-like diffusion takes place, as seen in η; and this continues, growing fainter and less perceptible for an hour, or sometimes an hour-and-a-half, until it wholly disappears, and all trace of nucleus is gone, the mass of the plasm of the combined cells being faintly granular, or perhaps reticulated; but the trailing flagella (θ) fuse together and unite with the body, which now rapidly changes, so that in the course of fifteen minutes it assumes the shape and condition shown in Fig. 11, N. At the end of four hours a kind of pulsating movement shows itself in this triangular sac, and at length it burst one or two, and sometimes (as in Fig. 12, *o*) the three corners of the triangular sac, and an enormous number of semi-opaque and extremely minute particles, escaped (Fig. 12), and by watching these they were seen to pass by growth into forms corresponding to the common form from which they arose.

These observations were made in the original and renewed study of the behaviour of this organism as a whole; but during the past three years the greatly superior microscopic apparatus, both illuminating and image-forming, placed at our disposal, has led to an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X 5,000.
23
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X 5,000.

PLATE II.

DESCRIPTION OF PLATES.

PLATE I

Fig 1.—*A Heteromita rostrata* (Kent)

„ 1.—*B. Monas Dallingeri* (Kent)

„ 2.—*C, D. H. rostrata* undergoing fission. This was seen in earlier studies to begin in the body, and apparently afterwards (as in *c, d*) to extend to the nucleus

„ 3.—Almost complete division of the body and the nucleus of the same; but a fibrous connection exists between the two parts of the nucleus.

„ 4.—Total severance of the nucleus, and the division of the body and the flagella is almost complete

„ 5.—*A H. rostrata* produced by fission

„ 6.—The final product of a long series of fissipartitions undergoing the change preceding sexual fusion.

Figs 7, 8.—The process of fission in this special form.

Fig 9.—The initiation of fusion in an ordinary form with one of the above

PLATE II

Fig 10.—Early result of fusion of two of these forms; the nucleus has blended, and exhibits a radial condition

„ 11.—A sac resulting from the complete genetic fusion of the two individuals

„ 12.—The ripe sac bursting and emitting minute semi-opaque spores

„ 13.—A study with great magnification of the nucleus separately. This represents the nucleus before any evidence of change is discoverable

„ 14.—The first evidence—seen only in the nucleus—of approaching fission in the organism; beaded threads appear throughout the plasm; and a line shows itself incident with the long diameter of the organism.

„ 15.—The fission begins in the body at *a*, but has been preceded by a condensation of the beaded structure to the right and left of the division-line

Figs. 16, 17, 18.—The nucleus seen in process of fission, almost divided (as seen in Fig. 3, pl. i.) and immediately after division

Fig 19.—Nucleus after division in the exceptional form shown in Fig 8, *h*, pl i

„ 20.—Condition of the nuclei in the fused forms

„ 21.—*g, h, i.* Growing spores.

„ 22.—*j.* The spore grown to the size of a nucleus, a pause takes place in actual enlargement by growth, and internal development of the beaded structure arises as at *k*

„ 23.—After the internal development of the nucleus the body begins to great apparently from the nucleus, and the flagella, *l, m*, are pushed out; after this, growth to the normal size is rapid

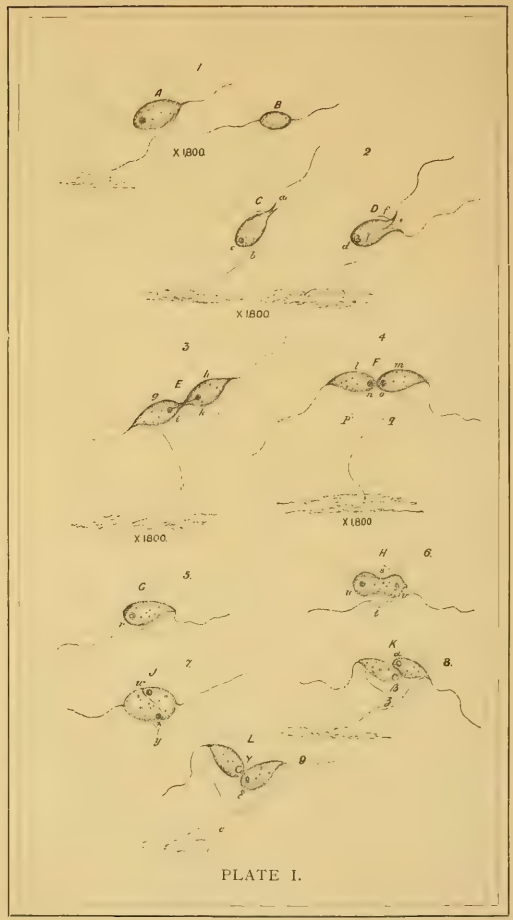

PLATE I.

endeavour to study alone the behaviour of the nucleus, both before and during, as well as after, the various steps in the cyclic changes undergone by this cell.

A magnification of from 5,000 to 6,000 diameters was found efficient for this, and with the life-history of the cell known, it was less difficult to trace by themselves the nuclear changes.

In studying these, it is only the changes occurring and observed in the living organism, and during its life, that have proved of any real value. The effect of desiccating and staining these saprophytes was in all senses unsatisfactory; the details were too minute, or too delicate to admit of this treatment, for whatever method was adopted, the results, as compared with the living form in the same stage, were simply unintelligible and useless. But this could only be really seen by *comparison* of the results of the same phenomenon in the living organism, and in the dried and stained state.

Nevertheless, by using a one-and-a-half per cent. of acetic acid, to which varying quantities of methyl-green are added, a decided strengthening of the special nuclear features was effected. Thus the nuclear envelope seen in most stages of its activity is much strengthened, and so were the beaded threads embedded in the nuclear hyaloplasm; and little, if any, inconvenience could be seen to result in the organisms.

In the inactive stages of the organism, or when no cyclic changes were manifest, the nucleus was hyaline, or at least exhibited nothing but the faintest traces of a colourless reticulation when very critically examined. This simple state of nuclear hyaloplasm is seen in Fig. 13, representing a portion of the body of the organism and the nucleus, the latter magnified 5,000 diameters.

By steadily watching a form in this condition, it would in all probability be seen to become more marked in its granulation, and at length what appeared like a delicate thread of beaded structure became more and more visible, occupying at first the circumference and at length the whole plasm of the nucleus, as shown in Fig. 14; but when this was complete a distinct line, made more visible by methyl-green, appeared, as shown in the nucleus of that figure. No indication of any division anywhere in the body of the organism had yet appeared. Now, however, it soon becomes manifest that the complex foldings and twistings of the beaded thread in the nucleus condense on either side of it, leaving the dividing line in a hyaline space, as shown in Fig. 15; and now for the first time a line appears at *a* and opens, so that the fission of the *body* has begun.

After this the process is rapid, occupying only three or four minutes. The nucleus opens as in Fig. 16, where it is seen apart from the body, corresponding to the stage seen in Fig. 2, D, rapidly passing into the condition seen in Fig. 17, corresponding to the state shown in Fig. 3, E, and where the fine-beaded thread appears to be drawn out, connecting the still dividing nuclei; but in this

condition the convolutions of the beaded thread have again distributed themselves over each divided nucleus, and soon enlargement and total separation take place, as in Fig. 18.

In the divided organism containing this organism the division-line soon again appears in the nucleus, and successive fissions go on. But in the nuclei belonging to the condition of special fissipartition shown in Fig. 7, the nuclei present quite a distinct appearance. No wall is at all discernible in connection with it, and it is slightly larger than the ordinary form; it is highly refractive, and, on critical examination, does not show the exquisitely fine convoluted beaded thread of the usual nucleus, but dissociated white, and slightly darker, somewhat oblong granulations, as shown in Fig. 19. When in the usual order, in the act of fusion shown in Fig. 9, L, this nucleus comes into contact with the ordinary one (γ, δ), there is almost immediate "melting of either into other," the whole mass resulting being milky and almost opaque, taking a white star-like appearance, resulting apparently from the diffusion of the whole mass through the entire plasm of the fused organisms, for in the course of a short time it disappears, and, as in Fig. 11, N, a non-nuclear triangular body results.

The bodies ultimately emitted from this are of extreme minuteness, and partially opaque; and in their growth one of the most remarkable features appear.

In Fig. 21, if *g* represent the original spores as sent out into the fluid, *p* will represent their growth in form and relative size in an hour, *i* shows one of the same in an hour-and-a-half, while *j* (Fig. 22) gives the result of growth at the end of nearly three hours; but now very little advance in size is made; there is a pause, as if the form had been arrested in growth by death. But, on critical examination, there is found to be a very slight enlargement taking place, and with it the formation of the *internal* beaded thread-like convolution which may take thirty to fifty minutes to complete, as in Fig. 22, K.

From this time growth is rapid; but it appears to proceed from within the nucleus outward, as shown in Fig. 23, where a small part of the body-substance has arisen outside the nucleus, *n*, and, at this stage, the two flagella are seen as though emerging from the nucleus, as at *l*, *m*. The body-substance now rapidly grows until the adult form and size are reached, as in Fig 1, A, when the processes of the life-history once more repeat themselves.

It would thus appear that every point in the cyclic changes in this organism originate in, or are initiated by, the nucleus. Fission is not only indicated, but begins first in the nucleus, while the nuclear changes during fission are worthy of careful note. The change in the character of the nucleus in certain forms after fission is full of suggestion, and that their subsequent union with nuclei of the common type is equivalent to fertilisation, we can scarcely doubt. Not less interesting is the origin of the nucleus in the growing form, and its relation to the growth of the body.

The non-nucleated saprophytes (as Fig. 1, B) may, perhaps, in some sense, be looked upon as wholly nuclear, although I have been unable to discover anything but the most faint traces of structure within them. They present the constant appearance of the nucleus when in a totally inactive state, as shown in Fig. 13.

This appears to present the activities of the nucleus in its simplest condition as accessible to our present means. That there are phenomena far more delicate than those here recorded, we can scarcely doubt, but it would appear that it is in the unicellular organisms that the first lessons in cell-history may be learnt.

W. H. Dallinger.

II.

Are Great Ocean Depths Permanent?

IT has been suggested by the editor that English readers would like to hear my views on that much-debated question, the Permanence of Ocean Basins. I am rather at a loss how to deal with the subject, because this question involves so many difficult chapters in the history of our planet, and because I regret to see that discrepancy of views exists on fundamental principles.

Mr. Wallace begins by arguing from the principle that "on any large scale, elevation and subsidence must nearly balance each other, and thus, in order that any area of continental magnitude should rise from the ocean floor, ... some corresponding area must sink to a like amount." I venture with deference to reply that I cannot agree to this. It seems, on the contrary, as if two different types of movement had been going on since the first formation of the terrestrial crust. In the first place, there is folding, recently explained in a masterly way by Professor Lapworth, in his Address to the British Association. Secondly, there is the sagging-down or "effondrement" of smaller or greater parts of the crust, caused by the progressive diminution of the planet's radius. *This descent of parts of the earth's crust seems to be the true origin of the great oceanic basins.*

Sometimes the contour of the sunken area follows the trend of a folded mountain chain; at another time it may cut right across it. In smaller examples the outline very often takes a more or less irregularly circular or elliptical form. The descent of a considerable area, forming a large new depression, demands a certain part of the existing volume of oceanic waters for the filling of the new depth. The consequence is the sinking of the oceanic surface all over the planet, and the *apparent step-like rising of coast lines*. Thus is explained the apparently episodic elevation of whole continents, without any disturbance of horizontality, or the least alteration of the net of watercourses spread over the land. It is in this sense alone that a certain balance of "elevation" and "subsidence" might be conceded.

In the entire Pacific region the limits of the oceanic basin are traced out by the trend of long mountain folds. So it is from New Zealand and New Caledonia to the borders of Eastern Asia, to the Aleutians, and all along the western coast of both Americas. This is not the case in the Atlantic, nor in the Indian Ocean; here the

coasts are not outlined by folded structure, except in the arch of the Lesser Antilles, and in the corresponding short arch passing through Gibraltar, which serves to connect the mountains of north-western Africa with those of the south of Spain. These are what we may call the Pacific and the Atlantic types of oceanic regions.

Indian geologists have shown how the immense Asiatic mountain waves, moving southwards against the Peninsula, have been dammed back by the resistent Peninsular mass, the Korána Hills, and the wedge-shaped mass of Assam; so that they actually form distinct arches, separated by deep angles receding to the neighbourhood of Tank, north of Dera Ismail Khan, to the Upper Jhelum and Brahmaputra Valley. In this case we call the Peninsula the "Vorland."

Now cast a look on the map of the North Pacific, and compare the receding angles which mark the western and the eastern ends of the Aleutian arch, where it abuts against Kamschatka and North-West America. You will remark that this part of the Pacific is a "vorland," and homologous to the Indian Peninsula, whilst the Yellow Sea, Behring Sea, and others lie within the folded region. You may also examine the Mediterranean, and observe that the western half lies wholly within a curved and folded mountain chain (Apennines, Sicily, North Atlas, Gibraltar, Andalusian Cordillera), and that in the eastern half all the part south of Crete and Cyprus and east of Sicily lies on the African "vorland," and the rest on the sunken parts of the Tauro-Dinaric arch.

In the Atlantic region the mountain folds, as a rule, break off against the ocean (*e.g.*, Brittany coast, Devon and Cornwall, south-west Ireland), or else have their folds facing away from the ocean, as in the case of the Alleghanies, and all other folded zones on the eastern side of North America as far as Newfoundland. The folds disappearing in south-west Ireland and in Brittany so very much resemble those rising from beneath the Atlantic on the coasts of Nova Scotia and New Brunswick, that M. Marcel Bertrand has ventured to publish a sketch-map, showing these chains trending right across the Atlantic.

There exists a curious tendency for a depression or a sort of valley to form in front of the great folds facing the "vorland." For instance, the depressions of the African desert in front of eastern Atlas, the river valleys in front of the high Indian chains, and the Persian Gulf in front of the Zagros chains. Quite recently the Austrian exploring ship "Pola" found a depth of 4,400 metres near the south-west coast of Greece, near the front of the Dinaric arch; and some of the greatest oceanic depths show exactly the same position in front of the arches of Japan, the Kuriles, and the Aleutians with Alaska. This is the homology, for example, between the Ganges valley and the Tuscarora depth, both marking the limit of the folded ranges and the "vorland."

The structure of the earth's crust does not, therefore, tell us that

the great depths must be very old or permanent; so let us compare the character of the sediments near the existing coasts.

The Old Red Sandstone is an extra-marine formation; yet the Old Red Sandstone runs out into the sea in the Orkneys, reappears in the Shetlands, and the same palæontological and extra-marine character is known in Spitzbergen. Old Red exists on the north coast of Lapland, and on the White Sea. The plant-bearing beds of the Karoo run out to sea in British Kaffraria; they are repeated in India and Australia. The fresh-water beds of the Wealden pass over from England to the Continent; they not only reappear in Hanover, they run out into the Atlantic in the lower Charente, and on the coasts of Spain and Portugal. Why must the continent which formerly bounded all these vast fresh-water basins have been limited within the existing 1,000- or 1,500- or 2,000- fathom line? The breaking down of the bed of the Ægean Sea, described by Spratt and Neumayr, is of Post-pliocene date, for Pliocene fresh-water deposits form parts of the coast; and yet the depths go far beyond 1,000 fathoms. In 1892 the "Pola" measured 3,591 metres in lat. 35° 52 36", long. 29° 1' 24" E., quite near the south-west coast of Asia Minor, and close to the mighty Ak Dagh (10,000 feet); and this although the separation of the neighbouring island of Rhodes is so recent, that not only do the Pliocene fresh-water beds pass over from the Continent, but according to Bukowski also considerable masses of Middle Pliocene fluviatile conglomerates, originating in Asia Minor, have been deposited by a great river on this island.

Now suppose the existing quantity of oceanic water to decrease, say by evaporation into the ether, as Zöllner once thought, or in any other way; we might by this gradual diminution of the entire quantity attain a beach-line 500 fathoms below the present shores. The continents would appear so much higher, and dry land would extend. Plains would successively appear, more or less similar to Holland, and our present rivers Shannon, Seine, Loire, &c., would flow through these plains. In most cases the rivers would be caused to cut back their valleys, new transverse and parallel lines of erosion would appear, and the plain would be diversified into hills and valleys. The hills south of the Shannon would probably show the rest of those anticlines and synclines which dip below the ocean in south-west Ireland, and we should be able to see a greater part of the northern Armorican arch. The Scilly Islands would appear as another granitic laccolite within the continued Armorican region of Cornwall. The gneiss of Eddystone would come up within this northern Armorican arch, exactly as the gneisses of the Alps stand up within and behind the folded arches. In a similar way, in the south, the anticlines and synclines of French Armorica, which disappear north and south of Brest, would begin to be visible; but in the north-west of Ireland we should see a plateau, ending in a steep cliff, the abrupt boundary of a deep channel, separating the great island of Rockall

from the European continent. And all this varied new-born land would be green and full of life, and people would not be at all willing to believe that it ever was not so.

Then we would descend to the new shore, and one of our great masters would tell us that ocean depths are permanent, that is to say, from 1,500 fathoms downward, or from 2,000 fathoms. The general impression resulting from the study would be the same as now, but the assumed permanent level would be reduced by 500 fathoms.

We might invite our master to undertake an excursion with us; we would go to Scotland. An isthmus, some 1,200 feet high at the narrowest part—Sir Wyville Thomson's ridge—would lead us first to some isolated peaks, nearly 3,000 feet high, and over a rising country to the high peaks of the Faröes. We would observe land-born coal-beds between the great coulées of basalt. Proceeding further, we would travel to the north-west, over a broad tract of rocky land some 1,200 or 1,300 feet high; then first meet an isolated mountain of about 1,800 feet, and from there ascend to the volcanic mass of Iceland, where we should see those vast fields of lava, dotted with active volcanoes, and observe the long faults and open rents cutting through the masses of lava and trending across Iceland in a broad arch, first from south-west to north-east, then northward, beyond the volcano Askja to Husavik; and beyond this broken and breaking zone we might gain the great "effondrement" or "Kessel-bruch" of Faxafjord, beset all round by volcanoes and hot springs, from Snaefells Jokull to Reykjavik. In following the rents so well described by our indefatigable Thoroddsen, we might detect faulted plant-bearing beds, and recognise the equivalents of the Faröe coal-seams. If some younger and more impressible student be in our company he might well exclaim, in face of these plant-bearing beds, stretching on to Greenland and showing the existence of a vast dry land in late times, and in face of these rents and volcanoes: "Verily, Professor, the sagging-down of the North Atlantic is the most recent event; it is going on before our eyes; and as the highest mountain chains are the youngest, so also are the deepest parts of the planet the most recent." I fear I should not know how else to answer the student than, "Really, I do not know."

Now let us quit the coasts and examine the interior of a great continent.

Modern geology permits us to follow the first outlines of the history of a great ocean which once stretched across part of Eurasia. The folded and crumpled deposits of this ocean stand forth to heaven in Thibet, Himalaya, and the Alps. This ocean we designate by the name "Tethys," after the sister and consort of Oceanus. The latest successor of the Tethyan Sea is the present Mediterranean.

I asked Dr. Diener, recently returned from India, to give me his estimate of the thickness of the deposits in the Silakank region. Dr Diener answers: From Dhauli-Ganga Valley, between Gweldung

and Pethathali encamping ground above Silakank (19,265 English feet), to Sirkia River in Hundes ; a complete section from (Cambrian ?) Haimantas to the Gieumal Sandstone (Cretaceous), without a great discordance, gives, according to Dr. Griesbach :—

Haimantas	3,000—4,000 feet.
Lower Silurian	200 ,,
Upper Silurian	1,100 ,,
Devonian..	700 ,,
Carboniferous	1,200—1,400 ,,
Permian and Trias	3,600—3,900 ,,
Lias and Spiti Shales	1,400 ? ,,
Gieumal Sandstone	1,200—1,500 ,,
	12,400—14,200 feet.

The determination of the thickness of the Spiti Shales and Gieumal Sandstone is difficult, because these less-resistent beds are crumpled into local folds.

A parallel section across the Kurkutidhár range (Chor Hoti, about 18,000 feet), Shalshal encamping ground and Shalshal Pass (16,390 feet) to Hundes gives, without the Haimantas :—

Silurian	1,200—1,400 feet.
Devonian and Carboniferous	1,800—2,300 ,,
Permian and Lower Trias	200 ,,
Middle Trias	100 ,,
Upper Trias to Dachstein Limestone ..	1,400 ,,
Dachstein Limestone, Rhætic and Lias ..	2,200 ,,
Spiti Shales	1,000 ? ,,
Gieumal Sandstone	1,500 ? ,,
	9,400—10,100 feet.

These figures show that a great and deep ocean has been incorporated into the continent, and that the deposits of this ocean form part of the highest mountain ranges.

It may be remarked that within the eastern Alps Mesozoic limestones of different ages contain deep-water radiolarian chert. But the great and well-bedded masses of white Rhætic limestones of Austria betray distinct proofs of a continuous rising of the shore-line. It is also true that certain bright red enclosures within these white limestones seem clearly not to be red deep-sea clay, but true *terra rossa*, formed by atmospheric decomposition of the limestone ; so that these beds must have formed reefs in the ocean. Therefore it is at present difficult to say whether in the Alps the Tethyan Ocean did at any time attain the total depth of, say, 2,000 fathoms ; or whether deposits followed so rapidly and depression was so continuous that this was not the case.

The later Tethyan history, the recapitulation of the vicissitudes which led to the formation of the existing Mediterranean, forms certainly one of the most attractive chapters of historical geography. Marine deposits of Mediterranean type (Erste Mediterranstufe, Miocène inférieur) enter the Rhone Valley, surround the present site

of the Alps, and continue far away to the East, to Persia, and have been met with by Griesbach near Balk, on the Oxus. Then these deposits were folded, in the Taurus, in Asia Minor, in Switzerland. Afterwards came the sagging-down of certain parts of these folds, near Vienna, in Hungary, &c., and all that varied series of consequent events. After the first Mediterranean came the formation of an immense horizon of salt deposits, stretching from Wieliczka to Persia; then a second Mediterranean reaching far into the newly-formed depressions; then the appearance of vast fresh-water lakes, lasting through a long period of time till the breaking down of the Ægean land and the re-conquest of the Black Sea.

Look at smaller examples of such partial subsidences; see Margerie's instructive paper on the Corbières, showing the sinking down of the Pyrenees, Miocene beds passing beyond Narbonne, while south of Cape Leucate two more recent Pliocene "effondrements" form the Rousillon, described by Depéret, and the Golfe de Rosas.

But this is only part of the Tethyan history. Michelin's and Duncan's palæontological studies in the West Indies have revealed the European character of certain deposits. It is the "Gosau type" of the Cretaceous which appears in Jamaica, and the Castel-Gomberto horizon of Upper Oligocene (warm type of Sables de Fontainebleau) is known in several other isles. In regions still further off, one of our first masters, the venerable Dr. Philippi, has shown that the present molluscan fauna of the Chilian coasts is of quite recent origin, and that until the beginning of Quaternary times the European Mediterranean molluscan types stretched far down the western coast of South America. At the same time the Mesozoic deposits of Chili, and those recently discovered at Taylorville in California,[1] show purely European characters, and the Neocomian of Bogotá is the exact equivalent of that of Barrême.

These facts teach us that an ocean-bed existed, but that some coast-line, maybe only an interrupted line, once stretched across the present Atlantic, and permitted the Gosavian and Oligocene corals, and the Miocene shells also, to cross. I do not overlook the fact that Dr. Philippi himself, struck by the analogies existing between the flora of Chili and that of Europe, recently refused to accept the hypothesis of a "bridge" to Europe, and preferred to suppose that identical climatic and other external causes produced analogous and even identical species of terrestrial plants. I refer to what has been excellently said by Mr. Blanford on this theory, a few years ago, in his address to the Geological Society of London. I believe that the parallel correspondence of the marine faunas up to the Quaternary period gives a more correct clue to the correspondence of the existing terrestrial floras in Chili and in Europe.

So I think that we must not only concede the extinction of a great

[1] See Hyatt and Dillen on the Jura and Trias at Taylorville, California. *Bull. Geol. Soc. America*, 1892, pp. 369—412.

Palæozoic, Mesozoic, and Tertiary ocean in south-western Eurasia, but admit also great recent changes in the middle or southern Atlantic. Geological evidence, therefore, does not prove, nor even point to, a permanence of the great depths, at least in the oceans of the Atlantic type.

Let me remark in a few words that, although I believe in the possibility of the formation of large new depressions, I do not hold with the old opinion, lately taken up again by M. Faye, that the continued sinking of the ocean beds may force chains of mountains to appear all round. This view could only be propounded for the Pacific basin; but the Pacific chains are folded in the direction *towards* the ocean, and not *from* the ocean. They are easily divided into arches, each of which presents the *convex* side to the ocean, so that the Pacific everywhere presents the character of a "vorland."

Let me, at the end of this long note, allude to a broad biological fact. In the higher beings we see *lungs* always preceded by gills; so it is even with the human child. The adaptation for breathing our atmosphere is of a later date; and we conclude that the whole terrestrial air-breathing fauna is a *derived* fauna, derived from amphibious forms quitting shallow water. This fact evidently points to a long existence of dry land, long enough to permit this accommodation to be effected; the accommodation clearly has been going on since Palæozoic times. Still there exists no proof that individual continents always remained the same, and we even know for certain that such was not the case with by far the greater part of these continents.

A similar lesson is also taught by the *eyes* in all the higher organised beings of the deep sea. The optical apparatus of abyssal species is profoundly modified by the exceptional environment, while the normal types of eyes are met with in the same genera within moderate depths. Therefore, we must also regard these deep-sea forms as *derived* forms. The blind and blinded Trilobites of Cambrian beds, the blind Trinuclei and the widely-expanded eyes in certain species of Aeglina in Lower Silurian strata teach the same lesson. At the same time, they show that deep-water must have existed over Bohemia, and over a good many other Palæozoic tracts, and that the depths were considerable enough to call forth these same abyssal metamorphoses of the eye.

We might, therefore, rather be induced to infer that in Prepalæozoic times there may have existed a universal hydrosphere or panthalassa covering the whole of the planet. Only with the first appearance of dry land began the deposition of clastic sediments. The higher forms of life may have been developed in waters of moderate depth and may successively have spread to the sun-lit continents, and to the dark depths, while the slow elaboration of the existing inequalities of the terrestrial surface was going on.

But this elaboration is still in progress. I believe with Reyer,

Fisher, Jukes, and many others that the great depths are mostly covered with volcanic products, with lava and ashes forming immense plains and overlain eventually by the deposits of the abyss; but I see no reason why parts of the ocean or even of the dry land may not to-morrow sink to form new depths, or why we should believe that all the great ocean basins have been continuously covered by water since panthalassic times. So far as the Atlantic is concerned, there even exists some evidence to the contrary.

But all this is unripe fruit. Our scholars will some day know more than their masters do now; so let us patiently continue our work and remain friends.

EDW. SUESS.

III.

The Origin and Classification of Islands.

EVERY island has its history, and in the case of all but newly-formed volcanic islands or coral islets, every island has a double history, that of the island itself and that of its colonisation by the plants and animals which live upon it. The rocks of which an island consists will give us an insight into, though not always a complete knowledge of, its geological history; and a study of its living inhabitants will generally enable us to decide whether it has been colonised as an island or by direct former connection with a continent. Some biologists maintain that the fauna of an island will show whether it has ever been united to a continent or not, and this is the question which I propose to discuss in the present paper, because the answer to it involves some important inferences and conclusions.

There are many ways in which an island can be formed; it may be but a portion of a continent severed from the mainland by the erosive action of the sea; it may be the mountainous part of a country which has sunk beneath the ocean; it may have been thrown up from the floor of the ocean by volcanic action, or it may have been built up by the growth of reef-making corals. There are, however, only two ways in which an island can have been populated without the intervention of man; either it must once have been united to a continent and its inhabitants must be the descendants of those that then lived on that continent, or else its tenants must have been transported across the sea by the help of drift-wood, or by birds, or by winds and storms.

It is evident, therefore, that *in most cases* there is likely to be a certain relation between the geological structure of an island and the nature of its fauna and flora. Islands formed in the ocean, whether by direct upheaval, or by volcanic eruptions, or by coral growths, are not likely to possess a large assemblage of plants or of animals; they may be covered with vegetation, but the animals found on them must be the descendants of occasional waifs and strays. On the other hand, an island which has once been part of a continent will, if it remain large enough, continue to support a large number of animals, and these will generally include a certain number of Mammalia and Amphibia.

Islands have consequently been divided into two great classes—

oceanic and continental—which are defined by Dr. A. R. Wallace in the following terms :—

Oceanic islands are "of volcanic or coralline origin, usually far from continents, and always separated from them by very deep sea, entirely without indigenous land mammalia or amphibia, but with a fair number of birds and insects, and usually with some reptiles."

"Continental islands are always more varied in their geological formation, containing both ancient and recent stratified rocks. They are rarely very remote from a continent, and they always contain some land mammals and amphibia, as well as representatives of the other classes and orders in considerable variety."[1]

As general definitions framed for the purpose of describing the conditions which have governed and limited the geographical distribution of animals, these sentences are doubtless sufficiently accurate, especially as Dr. Wallace admits there are some islands which do not come very clearly under either of these categories ; but he proceeds to lay down a *canon* the truth of which is by no means so apparent. He says :—"The total absence of warm-blooded terrestrial animals in an island otherwise well suited to maintain them, is held to prove that such island is no mere fragment of any existing or submerged continent, but one that has been actually produced in mid-ocean. It is true that if a continental island were to be completely submerged for a single day, and then again elevated, its higher terrestrial animals would be all destroyed, and if it were situated at a considerable distance from land, it would be reduced to the same zoological condition as an oceanic island ; but such a complete submergence and re-elevation appears never to have taken place, for there is no single island on the globe which has the physical and geological features of a continental, combined with the zoological features of an oceanic island."

Seeing how little we yet know of the geology of distant islands, this is a statement which further knowledge may at any time disprove, and there is, even now, good reason to believe that it is contrary to facts. If this assertion can be proved to be incorrect, I shall claim to reverse Dr. Wallace's argument, and to maintain that inasmuch as an island does exist which combines the geological features of a continental island with the zoological features of an oceanic one, then we may assume that the submergence and re-elevation of a continental island can take place, and, consequently, the absence of mammals in an island *cannot* be held to prove that it has never been united to a continent.

Considering the many subsidences and upheavals which are known to have occurred along the borders of continental areas since the beginning of Tertiary time, it would indeed be strange if some tracts, isolated by subsidence, had not been completely submerged for a time, and afterwards raised afresh from the sea. The West Indian

[1] "Island Life," by A. R. Wallace, second edition, 1892, p. 243.

region is one where such an occurrence is very likely to have happened, for deep-water deposits of late Tertiary age occur in many of the islands; while the raised coral-reefs which are found in the same islands, and reach up to a height of 1,800 ft. above the sea, prove that there has been recent upheaval to at least that extent.

Many of the smaller islands are volcanic, and may have been thrown up at any time; but Barbados, the most westerly of all the islands, has just the features of which we are in search; in its faunal aspect it is decidedly oceanic, while its geological structure is a curious combination, being partly continental and partly oceanic. The facts of the case are so remarkable that a brief review of them may here be given.

Barbados stands on a submarine bank or ridge which slopes away in every direction till a depth of more than 1,000 fathoms is reached. The core and base of the island consists of stratified rocks, ordinary sandstones, clays, and limestones, such as are formed in shallow water near a coast-line where rivers of some size carry detritus into the sea, and these strata must have been deposited very near such a shore, for many of the sandstones are composed of large quartz grains, which would not be carried far from land. Above these shallow water strata lie deposits of a totally different character, consolidated oceanic oozes, like those which are now found only in the deeper parts of the ocean, and are known as Globigerina Ooze, Radiolarian Ooze, and Red Clay. All these kinds of ooze occur in Barbados, and there is not only a superficial resemblance between them and the modern oceanic oozes, but a complete identity of structure, and a close analogy in chemical composition; upheaval and exposure to rain and weather have, of course, effected some little alteration, but have not obscured their structure.

It is certain, therefore, that the shallow sea and the extensive shore-line which it bordered sank to a very great depth, certainly to more than 1,000 fathoms, and probably to as much as 2,000 fathoms (12,000 feet). The site of Barbados was then part of the ocean-floor, but after a time upheaval took place, and it was gradually raised till it came within the sphere of reef-building corals; a small coral islet was the result, but as the upheaval continued, the earliest reefs were raised above the sea, and the area of the island was gradually enlarged. This process went on till the island attained its present dimensions (about the size of the Isle of Wight), the soft oceanic deposits and the still older sandstones and clays being protected from the erosive action of the waves by a thick coating of coral rock, except over a small area in the north-west part of the island, where the rain has cut deep valleys through the stratified rocks, and by carrying sand and mud into the sea has prevented the growth of continuous coral reefs on that side.

Now, an island with such a history must necessarily have received its present fauna and flora in the same casual way as an

oceanic island that had never formed part of a continental area. Accordingly, though Barbados is exceedingly fertile, and though the island when first discovered was clothed with forest and underwood, its native terrestrial fauna is a very small one.

There are only two mammals in Barbados which have been supposed to be indigenous, a monkey and a racoon-like animal, but I am informed by Col. Fielden that the monkey proves to be the Green Monkey of Western Africa, *Cercopithecus callitrichus*, and the "racoon" is a South American animal (*Procyon cancrivorus*). The monkey was doubtless brought over in slave-ships, and as it is known that the Caribbean Indians frequented the island before it was colonised by Europeans, and as the early settlers had intercourse with the colonists of Guiana, it is quite possible that the Procyon was introduced by man.

There are no indigenous Amphibia, but there are two Snakes and four species of Lizards. One of the snakes is a species peculiar to Barbados, the other may have been introduced by human agency from some of the other islands. Of the lizards, three are South American species, and the fourth is found in all the Lesser Antilles, though it is not yet known from South America. The manner in which reptiles may be landed on an island like Barbados is illustrated by the case recorded by Col. H. W. Fielden in the "Zoologist" of 1888, p. 236; this was the landing of an alligator on the shore from a floating tree-trunk, actually witnessed in 1886; it had doubtless been transported from one of the great South American rivers, but it was promptly dispatched by those who witnessed its arrival.

In a paper on the birds of Barbados, Col. Fielden remarks that, so far as he can judge, "the mammals, reptiles, and land molluscs owe their introduction either to ocean-currents, accidental occurrences, or to the direct agency of man, and a review of its avi-fauna does not point to a different conclusion." He also speaks of Barbados as a "truly oceanic island in the sense of its never having formed part of a continent since the introduction of its present meagre fauna," nor "since it emerged as a coral-reef from the ocean." This is perfectly true, but yet it does not come under Wallace's definition of an oceanic island for the reason already stated.

The difficulty of drawing hard and fast lines between oceanic and continental islands is also illustrated by the structure and fauna of the Seychelles Archipelago, in the Indian Ocean. These islands are surrounded by water of more than 1,000 fathoms, and are 850 miles distant from the coast of Africa. They might, therefore, be expected to exhibit all the features of oceanic islands; the facts, however, are as follow: The larger islands consist entirely of granite, and granite is a deep-seated rock which can only be exposed by the prolonged and repeated processes of erosion which take place on large areas of land. Dr. Wallace admits them to be remnants of

"a very extensive island," though he doubts whether this island ever formed part of a continent.

Turning to their zoology, we find that they are entirely destitute of mammals, but that they possess Amphibia, having two species of frogs and three species of Cæcilia, snake-like creatures, which burrow underground in the manner of worms. Now, it seems impossible to explain the presence of these Amphibia unless, at some remote period, the islands formed part of a continent, for salt-water is fatal to them, and destroys even the eggs of frogs. On the other hand, if the former connection with a continent be admitted, what can be said of the absence of mammals, for one would have expected that some of the smaller genera, such as rats, mice, civets, lemurs, and insectivores would have survived. Dr. Wallace suggests that the islands have at some time been so nearly submerged that the portions remaining above water were too small to support the existence of the smallest mammals. If this be the explanation, and if we accept the evidence of the Amphibia as to ancient continental connection, then the absence of mammals in such islands cannot be taken as proof that they have never been part of continental land.

New Caledonia, again, in the Pacific, is regarded by Dr. Wallace as an oceanic island. There are no indigenous Mammalia or Amphibia, the solitary frog being known to have been introduced. There are several peculiar lizards and a snake (one of the Boas); and the island is separated from neighbouring groups by water of more than 1,000 fathoms deep. Notwithstanding this limited vertebrate fauna there is evidence that New Caledonia has once been part of an extensive land area. Stratified rocks, believed to be of Secondary and Tertiary age, enter into its geological structure, and there is a genus of land snails (*Placostylus*) which occurs in the neighbouring archipelagoes, as well as on Lord Howe's Island and in New Zealand. Hence it has been recently argued that the unity and limitation of the *Placostylus* area can only be explained by the supposition that these islands are portions of a broken-up and submerged continent which was distinct from Australia. To reconcile such a supposition with the absence of mammals, it is only necessary to assume that this continent dates back to a time when mammals were not in existence in the Pacific region, just as Australia dates from a time when only marsupial mammals were in existence.

The cases above mentioned show that there is no constant relation between the geological structure of oceanic islands and the manner in which they have received their present inhabitants, for there are islands which are not far from a continent, and have clearly formed part of the continental area within the limits of Tertiary time, and yet are without any indigenous Mammalia or Amphibia. There are other islands which are destitute of Mammalia and are geographically oceanic, but nevertheless, have a geological structure of continental type. In other words, there are islands which testify to the former

existence of continents in what are now oceanic areas; and when all the facts are considered it is seen that they are opposed to the extreme views regarding the permanence of oceans and continents which have been accepted and maintained by Dr. Wallace in his "Island Life."

It would appear, therefore, that if the division into continental and oceanic islands be retained, a fourth class must be admitted into the classification; not only must continental islands be divided into ancient and modern, but oceanic islands must be defined geographically and then divided into those which are the remnants of sunken continents and those which are of recent origin. It will, however, be very difficult to distinguish islands of recent formation from mountain peaks which have been submerged and again elevated above the ocean level, for lofty mountain peaks so often consist of volcanic rocks.

I am convinced that the attempt to exclude islands in which stratified rocks occur from the category of oceanic islands will only lead to confusion and misconception, and that it is a mistake to infer from "the absence of warm-blooded terrestrial animals in an island otherwise suited to maintain them," that the island is of recent formation. The characters of an island fauna may, perhaps, be relied on to show whether the island has been *colonised* by former connection with a continent or not, but beyond this it will be no guide to the geological history of the region. The biological evidence must simply be taken for what it is worth, and the geological history of the island must be read from its geological structure, without the bias given by any preconceived theoretical ideas about the permanence of oceans and continents. There is, in fact, no hard and fast line to be drawn between oceanic and continental islands.

A. J. Jukes-Browne.

II.

NOTE ON MR. JUKES-BROWNE'S PAPER.

The Editor having kindly sent me a proof of Mr. Jukes-Browne's paper, I beg to make a few remarks thereon.

I cannot but think that Mr. Jukes-Browne's criticism of the Darwinian classification of islands, which I have adopted and more fully developed, is rather one of words and definitions than of realities. The very terms of the classification—"Oceanic" and "Continental"—show that it is a broad and wide-reaching one; and its main implication, the permanence of oceanic and continental areas, is equally broad and fundamental. That there should be islands situated upon the ever-fluctuating margin of these two areas which are difficult to class, or which may, at different geological periods, have possessed the characteristics of "oceanic" or of "continental" islands, is what might certainly be expected; the wonder is that there are so very few of them. Barbados is, technically, an oceanic island; but it is

situated upon the old sea-margin of the American continent, and a portion of that old continental margin forms the base of its oceanic deposits. I recognised the possibility of such a base for some apparently oceanic islands in the passage quoted by Mr. Jukes-Browne, but was not then aware that any such existed. Of course, if old stratified rocks could be shown to form the base of any of the mid-oceanic islands, the whole classification, and the theory which is founded on it, might be imperilled; but this has not yet been done.

It is evident that, with island groups whose components vary in size from many thousands of square miles to small sea-washed rocks, all definitions must be taken broadly and as applying to the group. Even among the pre-eminently continental British Isles there are many hundreds—out of the thousand of which they are said to consist—which have neither mammalian, amphibian, nor reptilian inhabitants; but it will hardly be objected that such cases as these upset the biological definition of continental islands. In the same manner the Seychelles are classed as belonging to the Madagascar group, and are, therefore, ancient continental, while Mauritius, Bourbon, and Rodriguez are true oceanic islands.

I do not know why Mr. Jukes-Browne should say that I regard New Caledonia as an oceanic island. At p. 473 of *Island Life* I refer to it as probably once connected with New Zealand; and again, at p. 485, I suppose it to have once formed an extension of New Zealand, which, though in some respects anomalous, has all the main characteristics of a continental island.

Looking at the question broadly, as a generalisation applying to all the well-marked islands and island-groups of the globe, I entirely deny the validity of the conclusions expressed in the last three paragraphs of Mr. Jukes-Browne's paper, conclusions which are founded exclusively on islands situated upon the margin of the continental area.

Alfred R. Wallace.

IV.

Biological Theories.

III.—THE RECAPITULATION THEORY.

DIRECT observation has shown that, when an animal species *varies* (*i.e.*, *becomes* unlike what it was before) in adult structure, those stages of the development which are nearest the adult undergo a similar but usually smaller change. This is shown in domestic species by the observations of Darwin, and the result is in exact harmony with the well-known law of Von Baer, which refers to natural species, both nearly related and very widely dissimilar.

Von Baer's observations as well as Darwin's, and as well as those of every student who has ever compared the embryos of two vertebrate-species, may be summarised as follows:—

Animals which, though related, are very unlike in the adult state, resemble each other more closely in early stages of development, often, indeed, so closely as to be indistinguishable in those early stages. As development proceeds, *in such species*, the differences between the two embryos compared become more and more pronounced.

If similar comparisons could be instituted between an ancestral species and its much modified descendants, there is no reason for doubting that a similar result would be reached. This, indeed, has been done in the case of some breeds of pigeons, which we have excellent reasons for believing to be descended from *Columba livia*. True, *C. livia* is not a very remote ancestor, but I do not think that will vitiate the argument. Let me quote Darwin verbatim: "As we have conclusive evidence that the breeds of the Pigeon are descended from a single wild species, I have compared the young within twelve hours after being hatched; I carefully measured the proportions (but will not here give the details) of the beak, width of mouth, length of nostril and of eyelid, size of feet, and length of leg in the wild parent-species, in pouters, fantails, runts, barbs, dragons, carriers, and tumblers. Now some of these birds, when mature, differ in so extraordinary a manner in the length and form of beak, and in other characters, that they would certainly have been ranked as distinct genera if found in a state of nature. But when the nestling birds of

these several breeds were placed in a row, though most of them could just be distinguished, the proportional differences in the above specified points were incomparably less than in the full-grown birds. Some characteristic points of difference—for instance, that of the width of the mouth—could hardly be detected in the young. But there was one remarkable exception to this rule, for the young of the short-faced tumbler differed from the young of the wild rock pigeon and of the other breeds in almost exactly the same proportions as in the adult state." ("Origin of Species," 6th edition, p. 392.)

As, it seems to me, would be expected, the differences between two more-distantly related animals at a stage corresponding to the time of hatching in pigeons is very much greater. A bird is obviously a bird at the time of hatching, and often the species and even the breed (as in the case just quoted from Darwin) are readily recognised at that time. A lizard or other reptile at the corresponding stage is easily recognised as a reptile. As shown by Von Baer the distinction between the bird and the reptile becomes more and more difficult as we examine earlier and earlier stages, and in the earliest stages, *i.e.*, in the ovum, it is often quite impossible to say even to which great phylum of the animal kingdom the ovum belongs.

The variation which produced birds from reptilian ancestors may be taken as a type of the kind of variation which is most familiar to us, *i.e.*, the variation affecting chiefly the adult animal.

Another kind of variation is illustrated by the three genera of gnats, *Culex*, *Corethra*, and *Chironomus*. Whatever may have been the form and structure of the most recent common ancestor of these three, the variation which has produced these from that ancestor appears to have affected the larval stages more markedly than the adult stages. These three are quite unlike in appearance in the larval stage. The mode of formation of the imago within the larva in *Chironomus* is quite unlike that in *Culex*. The three larvæ differ not only in appearance, but in internal structure. In the adult stage, though easily distinguishable, they are very much more alike in form, and even in internal structure, than in the larval stages.

We may not know the exact course of evolution of these three, but we may at least say that the variation in the average structure which has occurred in their evolution has led to a greater difference in larval structure than in adult structure.

Two kinds of variation must, therefore, be recognised—that affecting chiefly the adult structure, and that affecting chiefly the structure of the individual in early stages of development.

My object now is to show that in neither case can a record of the variation at any one stage of evolution be preserved in the ontogeny, much less can the ontogeny come to be a series of stages representing, in proper chronological order, some of the stages of adult structure which have been passed through in the course of evolution.

This would seem to be obvious enough, and there would appear to be no need for any argument to establish the view here put forward, viz. :—that the ontogeny is not an epitome of the phylogeny, is not even a modified or "falsified" epitome, is not a record, either perfect or imperfect, of past history, is not a recapitulation of the course of evolution. My excuse for urging what appears to me to be an obvious fact, is the existence of a statement the direct opposite of this view in almost every zoological text-book which has been published within the last fifteen or twenty years, as well as the frequent urging of the recapitulation theory in our colleges and universities, in the University Extension Lectures, and in a recent lecture given to the British Association, and even in the presidential address of one of our most eminent embryologists to the Biological Section of that Association.

Incidentally, it may be well to point out the restricted sense in which I use the word "variation." To vary, is, I believe, to *change* or to *become unlike*, whatever the thing varying was previously like. To *be unlike* is *to differ*; *not to vary*. The unlikeness observable among the members of a species is variety, or difference, or unlikeness, but it is not variation. Variation is change in the average constitution of successive generations of a species leading to the production of a new species, or race, from an old one. I have myself used the term in another sense in a previous paper in this series, but that is no reason why I should continue to use it in a sense which is liable to lead, and indeed has led, to confusion.

The terms "vary" and "differ" as above defined are not even partially synonymous. Difference and variation are respectively statical and dynamical (if I may use that term to express the opposite of statical).

In order that any structure of the adult which varies, and hence ceases to exist as an adult structure at all, may become an ontogenetic record of that adult structure, it is necessary that variation should occur in a way utterly unlike the way in which it does actually occur. The more the adult structure comes to be unlike the adult structure of the ancestors, the more do the late stages of development undergo a modification of the same kind. This is not mere dogma, but is a simple paraphrase of Von Baer's law. It is proved true, not only by the observations of Von Baer and of Darwin, already referred to, but by the direct observation of everyone who takes the trouble to compare the embryos of any two vertebrates, provided only he will be content to see what actually lies before him, and not the phantasms which the recapitulation theory may have printed on his imagination.

In order to produce a "record" it is necessary that new chapters be added at the end of the pre-existing record. It is necessary, in fact, that as the adult structure varies in one direction, the late stages of development shall vary in another, so as to become, not more like the new adult structure than they were before, but more like the old

one. If the variation is a mere increase in the definitive size of an organ which has hitherto not only increased in size in the evolution of the species, but also has *grown* during development of the individual, then, of course, a variation affecting the rudiment of this organ at all stages of development in which it is present, and causing it to grow more rapidly, will render the late stages of development (as far as that organ is concerned) more like the adult stage of the ancestor. This, however, is a mere result of the mode of growth of the particular organ ; its smaller size in late embryonic stages than in the adult is not a result, still less is it a "record" of the course of evolution.

The antlers of stags will serve as an example. Each stag develops a new pair of antlers in each successive year, and each pair of antlers is larger than the pair produced in the previous year. This yearly increase in the size of the antlers has been put forward as an example of an ontogenetic record of past evolution. I, however, deny that it is such a record. The series of ancestors may have possessed larger antlers in each generation than in the generation before it. It is not an occasional accidental parallelism between the ontogeny and the phylogeny which I deny, but the causal relation between the two. Had the ancestors had larger antlers than the existing ones, there is no justification for the assumption that existing stags would acquire antlers of which each pair, in later years, would be smaller than those of the previous year. The yearly increase in the size of the antlers is itself a character determined by Natural Selection. Phylogeny appears to have run parallel with the *pre-existing* Ontogeny. There are many breeds of hornless sheep, but they do not bear large horns in early years and then shed them. If a rudiment ever appears in the embryo of such sheep, its growth is very early arrested.

So it is in all the alleged cases of recapitulation. The gill-arches and clefts, and the blood-vessels of an embryo bird or mammal, present that striking resemblance to the corresponding parts of the *embryo* of a fish which is expressed in Von Baer's law. They differ, perhaps, only very slightly indeed. They are the modified representatives of the embryonic structures of a common ancestor. Whether they were, in that ancestor, the rudiments of gill-arches and clefts, &c., like those of an adult fish, or not, cannot be decided by embryological study. All we learn is that what now serve in their modified forms as the rudiments of the gill-arches of a fish, and of certain parts about the throat of a bird or a mammal, are so similar in early stages of development as to show that these parts are homologous. The greater resemblance of them to the adult structures of a fish than to those of a mammal may justify the belief that they served in a common ancestor as the rudiments of adult structures more like the adult structures of a fish than of a mammal, and that is all: they do not prove even that.

The statement that this stage in the development of a bird or a mammal is the modified remnant of the *adult* structure of the ancestor; and that the ontogeny is even in part made up of a series of remnants of ancestral *adult* structures arranged in chronological order, is not only unjustified, but is demonstrably false. If any generalisation in the whole science of zoology is borne out by fact, it is the law of Von Baer with reference to animals living free only in later stages of development. That law claims a parallelism between the development of a fish and of a bird which is quite inconsistent with the recapitulation theory, and completely consistent with the observed facts.

The early stages of the fish embryo are very like those of the bird embryo. These two *do* correspond to each other. The statement that the embryonic structure of a bird follows a course which is, from beginning to end, roughly parallel with, but somewhat divergent from, the course followed by a fish, is borne out by the actual facts. A bird does not develop into a fish and then into a reptile and then into a bird. There is no fish stage, no reptile stage, in its ontogeny. The adult resembles an adult fish only very remotely. Every earlier stage resembles the *corresponding earlier stage* of the fish more closely. There is a parallelism between the two ontogenies. There is no parallelism between the ontogeny and the phylogeny of either a bird or any other animal whatever. A seeming parallelism will fall through when closely examined.

Each transient stage in the development of any individual is a modification of the corresponding stage of development of its ancestors. It is in no case a modification of the adult stage of the ancestor. The adult stage of a bird, and no other, corresponds to the adult stage of the fish-like ancestors (if it ever had such ancestors).

The stalked "pentacrinoid" larva of *Antedon* (=*Comatula*) is a modified equivalent of the stalked *larva* of the "pentacrinoid" ancestor (if ever there was such an ancestor), and not the modified equivalent of the adult ancestor. The possession of a stalk in early stages of development appears to be an advantage, and hence the specific constitution which determines the development through a stalked stage has been preserved by Natural Selection. There is no evidence whatever to justify such mystical conceptions as those involved in even the most reasonable forms in which the recapitulation theory has been applied to this case.

The promise which the theory gave of serving as the guide to knowledge of past history, without the labour involved in palæontological research, was, indeed, tempting: and when the "royal road to learning" had been shown by it, it is not surprising that some zoologists should have entered for the race along this road. To what goal that road has led may be learned by a comparison of the numerous theories as to the ancestry of "chordata" which have been put forward by those who adopted the theory without enquiring as to its validity.

In this connection it may be not out of place to conclude with a quotation from Fritz Müller :—

"A false conception, when the consequences proceeding from it are followed further and further, will sooner or later lead to absurdities and palpable contradictions." (Opening sentence of chapter ii., "Facts for Darwin.")

That is precisely what the "recapitulation theory" has actually led to.

C. Herbert Hurst.

V.

Recent Observations on Fertilisation and Hybridity in Plants.

THE object of the following paper is to bring before the readers of NATURAL SCIENCE the most recent results which have been attained by workers in that department of Plant Physiology which relates to the actual process of the fertilisation of the female by the male element, and of the secondary processes by which the access of the active to the passive element is assisted. The enquiry naturally divides itself into two branches: (1) The nature of the process itself; (2) The subsidiary phenomena. The first of these enquiries evidently goes to the foundation of the laws on which depends the succession of life on the earth. Observations on this head, to have any value, must be carried out by experts of great knowledge, and with trained skill in the use of the most delicate microscopical appliances; the lowest forms of life, as well as the highest, must be put under requisition to yield up their secrets. As we ascend in the scale of organised beings, the various vital processes become more complicated, and secondary phenomena play a larger part in them; and it is in Flowering Plants that these become most interesting and most within the scope of the non-scientific observer. As, therefore, this will be the most familiar branch of the subject to those of our readers who have not made the physiology of plants their special study, we propose to commence with it.

The process by which the ovule of Flowering Plants is fertilised by the pollen-grain—*i.e.*, is acted on so as to be enabled to produce an embryo, without which no seed can germinate—is sufficiently well-known. The pollen-grain (the male element) must first be deposited on the stigma, where it puts out a pollen-tube which penetrates into the embryo-sac of the ovule, and there, coming into contact with one of the embryonic vesicles (the female element) fertilises it, the result being the growth of the embryo within the embryo-sac. The subsidiary phenomena are here the conveyance of the pollen-grain to the stigma, and the entrance of the pollen-tube into the embryo-sac. In the last-named process a great uniformity prevails throughout the Angiosperms or larger section of Flowering Plants; the only important exception being the

very interesting one recently discovered by Treub in the case of the Casuarineæ; but as that has already been fully described in these columns,[1] we will not further allude to it. Much greater variation exists in the phenomena connected with pollination, or the carriage of the pollen-grains to the stigma.

There is no more fertile source of error than the too rigid or too universal application of general laws. It is perfectly true that Nature "works not by partial, but by general laws." But a natural law is not, as some seem to suppose, an external force which directs the phenomena of life, it is simply a generalising of the phenomena themselves. A certain course of proceeding is of advantage to a particular organism or set of organisms; in other words, they are governed by a general law; but in some special case the same end is best attained by other means, and we find what is generally termed an exception to the general law. In no department of physiological botany is this variation more strikingly exhibited than in the phenomena of pollination. It was in 1793 that Christian Konrad Sprengel's celebrated work *Das entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen* was published. Centenaries have been celebrated on more trivial grounds than the appearance of this "epoch-making" work; since in it was first clearly laid down the law that in a large number of flowers the structure and arrangement of the male and female organs render self-pollination almost impossible, understanding by this term the pollination of the stigma by pollen-grains from anthers in the same flower. It is a matter of general knowledge how greatly this law was exemplified and extended by Darwin and his followers; but here again the tendency to a too universal application of the law has manifested itself, and during the last few years a remarkable number of observations have been made which indicate that self-pollination is far more general than had at one time been supposed. The perfume and the bright colour of flowers are undoubtedly important agents in attracting insects to assist in cross-pollination; and it has been assumed that this must always be their function. But that this is not invariably the case is certain. It has long ago been pointed out that the bee-orchis, *Ophrys apifera*, the flower of which so remarkably simulates a bee, is not visited by insects, and yet produces seeds abundantly. Very curious also are the facts in connection with the genus *Aristolochia*, in which the conspicuous pitcher-shaped perianth has been regarded as a contrivance for attracting insects and ensuring self-pollination. The walls of the pitcher are clothed with woolly or glutinous hairs, which have been assumed to have the function of detaining the pollen brought by the insects which enter the pitchers in great numbers from other flowers, and thus enable them to reach the stigma. W. Burck states, however, as the result of observations

[1] Natural Science, vol. i., p. 132.

on several species of *Aristolochia*, that when flies enter the chamber formed by the lower part of the perianth, and become dusted with pollen, in their efforts to escape again from the chamber they come into contact with these hairs, and thus lose, before they pass to another flower, almost every grain of pollen. In *A. barbata* at least 600, and in *A. ornithocephala* about 6,000 pollen-grains would be required for the fertilisation of all the ovules in an ovary. Out of a very large number of flies captured in these chambers, only a very few showed the presence of even a small number of pollen-grains adhering to them. On the other hand, both these two species and *A. elegans* are abundantly fertile when pollinated with their own pollen. He concludes, therefore, that these species at least are self-pollinated. Herr Burck also points out the interesting fact that in *Aristolochia* the true stigma and style are abortive, and that the so-called stigmatic surface consists of the connectives of the anthers which have coalesced by their sides into a cup, are provided on their margins with papillæ, and have assumed the functions of a stigma. In the instances just named, and in others of the same kind, it is an allowable hypothesis that the bright colour or sweet scent of the flower is a survival from cross-pollinated ancestors which has now lost its meaning. But this will not apply in other instances. The hazel and the larch have, it is true, unisexual and therefore necessarily cross-pollinated flowers. But they are not visited by insects, and are universally regarded as typical examples of wind-fertilised plants. What, then, is the object of the bright red colour of the styles of the one and of the scales of the opening cones of the other? It cannot be a survival, since the Corylaceæ and the Coniferæ are both unquestionably archaic forms of life. Bright colours, indeed, appeared very early in the evolution of plant-life. How are we to explain the brilliant red of the sporange of *Sphagnum*; or the bright pigment of the antherid or "globule" of the Characeæ; or the brilliant colour of species of *Peziza* or *Boletus*? Does Nature love beauty for its own sake?

Prominent among those who advocate the anti-Darwinian view of the prevalence of self-pollination, is Mr. Thos. Meehan, of Philadelphia, a botanist of great experience in the cultivation of plants. He adduces a large number of American plants in which he asserts this mode of pollination to be the rule, among others, in *Amsonia Tabernæmontana*, belonging to the Apocynaceæ, in which the flowers are showy and abundantly fertile, but their structure is such that no insect, not even a thrips, can gain entrance to the nectary. The mouth of the corolla-tube is so densely matted with hairs that if the pollen-clothed tongue of an insect were thrust through the mass, it would be completely cleansed; nor is there any room for the tongue to pass the capitate stigma. To effect pollination the anthers curve over and rest upon the stigma. Other examples of habitual self-pollination are given in *Symplocarpus fœtidus*, belonging to the

Aroideæ, in which the flowers are frequently proterogynous and as frequently proterandrous; in *Portulaca pilosa*, the flowers of which open only in the sunshine, and yet seed abundantly when grown in the shade; in *Cuphea Zimpani* (Lythraceæ), *Lopezia coronata* (Onagraceæ), in several species of *Lonicera*, *Phytolacca decandra*, *Lycopersicon esculentum*, *Hamamelis virginica*, &c. A large number of the American Compositæ he asserts to be self-pollinated. He has observed pollen-tubes entering the clefts of the bilobed stigma before it opens; and the pollen-grains, even when large and brightly coloured, frequently fall on the stigma before any insect can possibly enter the flower. Mr. Meehan, indeed, goes so far as to say that whenever a species is unusually productive, he finds, as a rule, arrangements for self-pollination. An Italian botanist, Dr. Terraciano, states that in many species of *Nigella*, notwithstanding the conspicuousness of the flower and the presence of nectaries, the structure is adapted for self-pollination. According to Warming, all the Greenland and Iceland species of *Euphrasia* are self-pollinated. Even so decided a Darwinian as Professor Caruel, of Florence, is inclined to doubt whether too much has not been taken for granted in the prevalent theory of the part played by the bright colour and sweet scent of flowers in attracting insects, seeing that, as far as we know, the visual and olfactory organs and perceptions of many animals are very different from our own. In particular, he calls attention to Sir John Lubbock's observations on the different effect of colours on many animals from that which they produce on us; to the eyes formed of facets and to the ocelli of insects, and to their sensitiveness to the ultra-violet rays of the solar spectrum. Rosen asserts that even with many wind-pollinated plants, such as *Carex* and *Festuca*, self-pollination is the rule.

On the other hand, the orthodox view receives support from a very large nnmber of fresh observations which reveal adaptations clearly intended to promote cross-pollination by the agency of insects. In a series of papers in the *Botanical Gazette* and elsewhere, Mr. C. Robertson describes a large number of observations to this effect on American plants. Professor Halsted states that the flowers of the barberry are very rarely, if ever, self-pollinated, and calls attention to the remarkable irritability of the stamens of species of *Portulaca*, which promotes the scattering of the pollen over the bodies of insects visiting the flowers. The structure of the flowers of *Arum*, *Dracunculus*, *Helicodiceros*, *Arisæma*, *Amorphophallus*, and other species of Aroideæ, has been made a special study by several Italian botanists. The extraordinary simulation by the open flower of the appearance and odour of decomposing flesh, appears specially designed to deceive and attract necrophagous coleoptera and diptera, which aid in the carriage of pollen. Schulz asserts that in the family Sileneæ of Caryophyllaceæ the proterandry is in many cases so marked as to render self-pollination impossible. Mr. G. F. Scott Elliot has, in the *Annals of Botany*, a very interesting paper on the

pollination of South African plants by birds. The Cinnyridæ, or sunbirds, are exceedingly good pollinators, especially *Nectarinia chalybea* and *bicollaris*, and *Promerops caper*. Like bees, they, as a rule, visit only one species of flower at a time. Mr. Scott Elliot believes that the identity of colour—an unusual shade of red—in the majority of ornithophilous flowers and on the breast of species of *Cinnyris*, is an important element in this pollination. Mr. H. N. Ridley calls attention to a remarkable structure in the flowers of *Bulbophyllum macranthum* and other orchids of Singapore for cross-pollination by the agency of insects. C. Correns describes the special arrangements for this purpose in *Salvia* and *Calceolaria*. Lists of cross-pollinated plants are given by McLeod from Belgium and the Pyrenees, by E. Loew and Knuth from Germany, by Delpino, from Italy, and by Scott Elliot from Madagascar. Kellgren enumerates 33 species of Leguminosæ growing on the Omberg, an isolated mountainous region in Germany, chiefly covered with pine woods, all pollinated by lepidoptera. The mode of pollination in the different species of *Yucca* is of great interest, and has long attracted the attention of botanists. Professor Riley, who has made it a subject of special study, states that in all the species examined by him, self-pollination is almost impossible; and that in each species the pollination is effected by a single species of insect. The pollinator of *Yucca filamentosa* is a moth, the female of *Pronuba yuccasella*. The object of the visit of the moth is the deposition of its eggs in the ovules of the *Yucca*, the coats of which it pierces. This is always effected as soon as the flower opens; and the moment an egg has been deposited the insect rushes to the anthers and carries a quantity of pollen to the stigma, stuffing it into the stigmatic cavity with its proboscis. Ten or twelve ovules may thus be destroyed, but the number in each ovary is so large that this does not practically affect its fertility. In Philadelphia, where the moth makes its appearance about the time that the *Yucca* is in flower, the latter produces abundance of seed, while it does not set its fruit in Washington and St. Louis, where it flowers a fortnight before the arrival of the moth. Finally, the report, in the *Botanical Gazette*, of a series of experiments carried on by Miss M. Reed, mostly on petunias, fully confirms Darwin's statement that cross-pollinated produce more seed-vessels than self-pollinated plants, and that the capsules are heavier.

Attention has long been called to the fact that not a very small number of plants produce, in addition to their ordinary showy flowers, others—termed cleistogamic—which never open, in which the corolla is partially or entirely suppressed, but which ripen abundance of seeds, being, of course, self-pollinated. A very good example of these cleistogamic flowers is furnished by the various species of "dog" violet, *Viola sylvatica* and its allies. The conspicuous flowers which appear in the early spring are mostly sterile;

but throughout the later spring and summer very inconspicuous flowers are abundantly produced, completely concealed in their calyx, and these almost invariably give rise to fertile capsules. It would appear as if nature provided in them a surety for the production of seed in case of the continued sterility of the early spring flowers. Several species of Leguminosæ produce cleistogamic flowers, borne, in some cases, on underground shoots, the seed-vessel never appearing above the surface of the soil, and other instances are furnished by the genera *Oxalis*, *Impatiens*, and many others. During the last few years considerable additions have been made, by Meehan and others, to the list of cleistogamic species, and these include several plants belonging to the British flora, such as *Polygonum acre*, *Hydropiper*, *Persicaria*, and *maritimum*, *Scleranthus annuus*, &c. In *Polygonum acre* and *Hydropiper* the cleistogamic flowers are very numerous, and appear always to produce ripe seeds; they are small and completely hidden in the leaf-sheaths. W. Burck describes several species of tropical plants which bear flowers that never open, and must, therefore, be self-pollinated, although coloured and scented, and producing abundance of nectar. This is strikingly the case with *Myrmecodia*, one of the genera which furnish abodes in their stems for colonies of ants; and he suggests the explanation that the flowers were at first adapted for cross-pollination, but that the visits of insects have been gradually suspended in consequence of the attacks of the ants which inhabit the tubers. A. Schulz connects the appearance of cleistogamic flowers with unfavourable climatal conditions. In *Tephrosia heterantha*, a native of the Argentine Republic, Hieronymus states that the pollen-grains are few in number in the cleistogamic flowers, and that their tubes pierce the wall of the anthers in order to reach the stigma.

The structure of the flowers in the genus *Vitis* is very interesting. In the native state all species of vine have two kinds of flower,—male, in which the pistil is subject to all degrees of abortion, and hermaphrodite, in which both stamens and pistil are fully developed. The stamens differ remarkably in the two kinds of flower. In the hermaphrodite flowers the filaments are short and curved backwards, so as to remove the anther as far as possible from the stigma; in the male they are longer and erect; but in the cultivated vines of Europe, all of which are varieties of *Vitis vinifera*, and have only hermaphrodite flowers, the filaments are long and erect, as in the male flowers of the wild plant. The pollen-grains of the two kinds of stamen also differ in their power of fertilisation. Millardet, who has given great attention to the phenomena connected with the fertilisation of the vine, states that in the wild state the vine is wind-fertilised, although the flowers have a powerful odour, the purpose of which is obscure. In the cultivated state he has observed abundance of two small coleoptera in the flowers, which may also probably take some part in the pollination. Kronfeld

states that the cultivated grape-vine is also occasionally pollinated by honey-bees.

As has already been pointed out, those flowers which are unisexual—*i.e.*, which have either pistil and no stamens, or stamens and no pistil—must necessarily be cross-pollinated. Schulz, however, states that in many of our trees which are normally unisexual, hermaphrodite flowers do occasionally occur. Thus, in the elder, hermaphrodite flowers or transitional forms between these and unisexual are frequently to be found at the base of the male catkins. Hermaphrodite flowers also occur in the birch, though less frequently, very rarely in the hazel; in the oak there are often ovaries at the base of the male catkins, and rudiments of stamens in the female flowers. The mode of fertilisation of the hazel is involved in great obscurity, since scarcely a trace of the ovules can be found in the female flowers at the time when the catkins are discharging their pollen. In the ash, all kinds of condition may be met with, male, female, and hermaphrodite flowers; and either the same or different kinds on the same tree. The ash is probably on the road to becoming completely dicœcious.

Few phenomena in physiology, whether animal or vegetable, are more puzzling than those of parthenogenesis, that is, the production of a normal fertile embryo without any preceding act of fertilisation. The example in the vegetable kingdom of *Cœlebogyne* is familiar to all students of text-books. Another very remarkable instance is now on record. There have been few more interesting contributions to botanical science during the last few years than Dr. D. D. Cunningham's "On the Phenomena of Fertilisation in *Ficus Roxburghii*," published in Calcutta in 1889, with beautiful illustrations. According to Dr. Cunningham the figs are in this species either male or female, all on the same plant being of the same sex. The male figs contain perfect male flowers, which produce pollen, and atrophied female or "gall-flowers," which never produce seed, but within the ovaries of which the eggs of an insect—usually a species of *Eupristis*—are deposited and develop into pupæ. The female figs contain perfect female flowers, in which the eggs of the insect are never found, and which produce fertile seeds. The terminal opening of both the male and female figs is so obstructed by a covering of bracts that they are almost completely closed chambers; and the perfect development of both the male and female flowers is dependent on the access of the "fig-insect" to the interior of the cavity, without which they do not arrive at a functional condition. Although the development of the embryo in the female fig is essentially connected with the access of the insects to the cavity, Dr. Cunningham believes that it does not depend on the introduction of pollen by their agency. The nearly entire closure of the opening by bracts presents an almost insuperable obstacle to the introduction into the female fig of a sufficient quantity of pollen for the fertilisation of every one of the very numerous ovules

by a separate pollen-grain; and but very few pollen-grains could be found in the female figs. Although it is possible that in some cases ordinary pollination may occur, the author asserts that the embryo is usually formed without any process of fertilisation, arising as an outgrowth of the embryo-sac. The full development of both the male and female flowers appears to depend on a simple hypertrophy of the tissues of the fig, resulting from the irritation caused by the female insect in the act of laying its eggs within the ovary of the "gall-flowers" of the male figs, and of their persistent attempts to do the same within the flowers of the female figs, in which attempts they are frustrated by the great thickness of the wall of the ovary. In connection with the fertilisation of the fig, it may be mentioned that Professor Riley has enumerated fourteen species of insect as taking part in the so-called "caprification" of the wild figs of North America. He recommends to the fig-growers of California the introduction for this purpose of *Blastophaga psenes*.

With regard to the connection between the duration of life of the individual and the mode of fertilisation, Meehan makes the general statement that flowers which are wholly dependent on insects for their fertilisation are always perennials, and that an innumerable number of their flowers fall unfertilised; while all annuals, on the other hand, can self-pollinate themselves when cross-pollination fails; and in almost all cases all the flowers of annuals are fertile.

The phenomena of hybridity in the vegetable closely resembles those in the animal kingdom. It is possible to fertilise an ovule by pollen belonging to a different species, but only if the two species are very nearly allied. It is very rarely that species which are fertile with one another can be placed in different genera. Hybridisation is one of the every-day resources of the gardener; but that cross-breeding occurs in nature has been doubted by some. There seems, however, scarcely to be room for doubt that in some of our abundant wild genera, such as *Rubus*, *Salix*, and *Hieracium*, hybridity is not uncommon in nature. It has long been known that in some genera, such as *Passiflora*, and in some Orchideæ, the ovules appear to be even more readily fertilised by pollen of a different species. W. Focke now states that this is also the case with the species of *Lilium* belonging to the group *bulbiferum*, and with some species of *Hemerocallis*; and J. H. Wilson affirms the same respecting the Cape genus *Albuca*, also belonging to the Liliaceæ. According to Millardet none of the so-called hybrid vines cultivated in Europe are true hybrids, *i.e.*, products of the crossing of distinct species; they all spring from the crossing of different races of the same species, *Vitis vinifera*. He further states that, in the vine, it is the male parent that exercises the preponderating influence on the descendants. Rimpau has carried out a series of experiments on the crossing of some of our most common agricultural plants. He has obtained ten artificial and nine natural hybrids in wheat, and also obtained a fertile hybrid between

wheat and rye belonging to different genera. Between different kinds of barley he obtained two artificial and six natural hybrids; in no case did he succeed in fertilising a two-rowed barley by pollen of a variety with a larger number of rows. Among oats, five natural but no artificial hybrids were obtained. Very few natural hybrids occur among peas ; with different varieties of beet crossing is much more common. Rimpau states that if a new form exhibits great variability among its descendants, it is probably hybrid; while, on the other hand, it is most likely a spontaneous variety if the descendants maintain great constancy.

The Darwinian hypothesis that a sexual mode of reproduction is absolutely necessary to the maintenance of the higher forms of life, and that continual propagation by non-sexual methods must result in deterioration and ultimate extinction, has not been allowed to pass unchallenged during recent years. Möbius in Germany and Meehan in America have pointed out the great length of time, extending in some cases to thousands of years, during which some plants have been continuously propagated by non-sexual methods without apparent deterioration or increased liability to disease. This is the case with many fruit-trees, such as the fig, date-palm, banana, yam, batatas, and olive, and with the Canadian water-weed, *Elodea canadensis*, imported into this country from America, the male plant of which is still unknown here; though it is asserted by others that this pest is now gradually dying out from our rivers and canals. On the other hand, the weeping-willow and the Lombardy poplar—varieties which never produce seed, and which are propagated solely by cuttings—do appear to be threatened with extinction, owing to their abnormal liability to disease.

We now come to the second part of our subject, and, as we have already said, the more difficult one—the nature of the process of fertilisation itself. In general terms, fertilisation, or fecundation, may be defined as the union of the elements of an active male with those of a passive female cell, the result being the production of an embryo which develops into a new individual. In the animal kingdom, the only mode of reproduction in all the higher forms is a sexual one, non-sexual propagation having survived only in the lower types. In the vegetable kingdom, the two modes work side by side in the higher forms, while in the lowest forms sexuality is unknown. It becomes, therefore, a problem of great interest to determine in what way the sexual mode of reproduction arose among plants. Much light is thrown on this question by the phenomena which have been observed among Algæ. A very common mode of propagation among our ordinary fresh-water Algæ is by means of non-sexual zoospores or swarmspores, minute flagellate bodies which escape from the cells of the parent plant, move about in the water with great rapidity, and then finally come to rest, withdraw their cilia, and develop into new plants. Among the brown sea-weeds, there is a great variety in the

nature of the reproductive organs: we have non-sexual flagellate zoospores; we have a rudimentary sexual process in the conjugation of zoogametes, flagellated motile bodies which closely resemble zoospores, but which have no power of germination without first uniting in pairs; and, finally, we have an advanced mode of sexual reproduction in which the male and female elements differ almost as widely as they do in the highest flowerless plants, the former being a minute motile multiflagellate antherozoid, the latter a much larger perfectly passive oosphere. But what is most interesting is that we find in the brown sea-weeds all sorts of intermediate conditions between these, leading us to the irresistible conclusion that all the higher developments of sexuality have had their first origin in the union of two motile flagellate masses of protoplasm between which there is no apparent differentiation; but that very early, as we ascend in the evolutionary scale, a differentiation sprang up between these two masses of protoplasm, which became gradually more and more marked as they developed into what we call male and female cells. The latter soon lost their flagellate character, became quiescent, and increased in size; the former gradually passed from a biflagellate zoogamete indistinguishable from a zoospore, to the multiflagellate antherozoid of the higher Cryptogams. The passage from these to the germinal vesicles in the ovule and the pollen-tube of flowering plants is somewhat more difficult to follow.

The production or non-production of sexual organs in plants is very much a question of external conditions. In a recent paper in the *Biologisches Centralblatt*, Professor Möbius has published a very interesting summary of our knowledge on this subject. An abundant supply of nutriment promotes the production of vegetative rather than of reproductive organs; hence the value of root-pruning in increasing the fertility of fruit trees. The conditions which, on the whole, favour the formation and fertility of the sexual organs are—a high temperature, with not too much moisture and not too much supply of nutriment, abundance of light, and, in the case of the fruit trees of temperate climates, a period of complete rest in the winter. Many of the fruit-trees of our climate will not flower in the tropics. The potato, which blossoms only sparsely with us, produces flowers every year in its drier native country of Chili. *Epilobium angustifolium* will flower with us only in sunny situations, and the brighter the light the deeper the colour of the flowers. The production of flowers and fruit has an exhausting effect on the plant. A well-known illustration of this is the "century plant," *Agave americana*, which flowers only once, when from 30 to 100 years old, and then dies. A very abundant fruit year is commonly followed by one of comparative scarcity.

A similar law prevails also in the lower forms of vegetable life; but with this difference, that we find there the much greater plasticity which is the great feature of a low type of life. Many of our

commonest Algæ will fructify only in conditions which are unfavourable to the production of their vegetative organs. From an exceedingly interesting series of observations made by Klebs on that beautiful organism the "water net," *Hydrodictyon reticulatum*, we learn that all the cells in a net are apparently equivalent, *i.e.*, are adapted to produce either non-sexual zoospores or sexual zoogametes, and that the tendency to produce one or other of these structures is largely a question of nutrition. In a single net, consisting of equivalent sister-cells, some of the cells can be excited, by external conditions, to develop zoospores, others to produce zoogametes, the formation of the former being, in this case, absolutely dependent on light. A similar law prevails in *Volvox*.

Professor Weismann, in his recently-published volume of "Essays," advocates the view that the old idea of an essential difference, at all events of an opposition, between male and female elements, between so-called "sperm-cells" and "germ-cells," must be abandoned; that they are essentially alike, and differ only so far as one individual differs from another individual of the same species; and that fertilisation is no process of rejuvenescence, but merely a union of the hereditary tendencies of two individuals. This view he supports from the results of experiments which he regards as demonstrating the remarkable fact that, even in animals as high in the scale as the Batrachia, the "sperm-nucleus" can be made to play the part of an "ovum-nucleus," and *vice versâ*; it being possible to produce a free-swimming larva in the formation of which no "germ-nucleus" has taken any part; but, as far as the vegetable kingdom is concerned, the weight of evidence does not appear to be in favour of Weismann's hypothesis.

As has already been pointed out, very early in the evolution of the lower forms of vegetable life the motile zoospores began to assume sexual functions, and to become converted into zoogametes, and these again into active male and passive female elements; and, although the plasticity which is characteristic of the lower forms of life renders this differentiation for a time unstable, it becomes gradually more and more firmly established. Even in that class of beautiful Algæ, the Conjugatæ, of which *Spirogyra* and *Zygnema* are examples, where the "conjugating" cells show scarcely any difference even under high magnifying powers, it has now been shown that there is an essential difference between the male and female cells in their physiological constitution, and in their behaviour towards one another. Still more decisive is the fact, established by Auerbach in the animal, and by Rosen in the vegetable kingdom, that the male and the female nuclei behave differently to staining reagents, the former belonging to the class of "cyanophilous," the latter to the class of "erythrophilous" substances; that is, they take up especially blue and red colouring substances respectively. Other histological differences have also been pointed out between the male and female nuclei,

such as the possession of nucleoles by the former and not by the latter.

The act of fertilisation or fecundation appears to consist in the actual fusion of the nuclei of the male and female cells; at least this is the view taken by one of the highest authorities on this subject—M. Guignard—from a very careful series of observations on *Lilium martagon* and *Fritillaria imperialis*, in opposition to the theory of Van Beneden and some other observers. This fusion is assisted by the action of remarkable bodies which have only recently come under observation, and which have been termed "tinoleucites" or "directing spheres." These bodies, which have been observed by Guignard and Van Tieghem in plants, and by Fol and others in animals, are minute spherical masses of protoplasm occurring in both the male and female cells, which appear to exercise the function of directing the course of the male nucleus so as to bring it into contact with the female element. There appears to be also some directing principle in the higher Cryptogams which governs the antherozoids in finding their way to the oosphere within the archegone.

Without dogmatising on so intricate a question, the theory may at least be hazarded that every act of fertilisation is simply a modification of a process of nutrition, the conveying to a potential germ of some property which it does not derive from its own ancestors, but which gives it greater completeness, and endows it with greater resources in the struggle for existence. Sufficient, at all events, has been said to show how rich a field there is still open to the skilled microscopist in the investigation of the interesting problems connected with the perpetuation of animal and vegetable life on the surface of the globe. While it is probable that no human researches will ever lead to a solution of the questions: "What is life?" and "How were the phenomena of life first infused into inorganic matter?", we seem, year by year, to be able more closely to follow the steps by which, from the first germs of life, has been gradually evolved that marvellous complexity in the animal and vegetable worlds, the laws of which excite our increasing wonder and admiration the more minutely we investigate them.

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ALFRED W. BENNETT.

VI.

Animal Temperature.

THE higher animals have within their bodies some source of heat and some mechanism to regulate the production and loss of that heat, for, equally in the height of summer as in the depth of winter, their mean temperature is constant. This fact was known to the Ancients, but imperfectly; they had no thermometers, no exact methods of determining temperature. They judged from their sensations, but here sensations are imperfect guides.

A patient attacked by an ague feels chilly, miserably cold; he huddles up by a fire, he shivers, and his teeth chatter; he says that he is cold, but at this precise moment he is in a fever-heat. How does this contradiction arise? The feeling of heat or cold arises in the nervous structures of the skin. When these nerve-endings are flushed by a rapid stream of blood, there is a sensation of warmth; should the blood supply be small, not enough to compensate for the loss of heat by radiation and conduction, there arises a sensation of cold. During the *rigor*, for such is the name given to the early stage of ague, the vessels of the skin are contracted, the skin is pale and cold, its temperature twelve or sixteen degrees below the normal blood-heat; but, at the same time, the temperature of the internal parts has risen six or seven degrees above the normal.

Let the converse condition be studied in the same disease during its third or *sweating* stage. The skin is now red and flushed, its blood-vessels are dilated; the patient is bathed in perspiration; he says that he is intensely hot, although his internal temperature is rapidly falling to the normal.

Animal heat the Ancients considered to be beyond the reach of physical or chemical laws. They could assign no cause for it, and therefore looked upon it as some innate quality, something essentially "vital." This "vital" heat was supposed to be concentrated in the heart, and to be distributed to the body by the blood in the veins; it was prevented from accumulating by respiration, the chief function of which was to cool or temper the blood.

After the year 1595 it was first possible to determine the temperature of the body more exactly, and to make observations upon the independence of the temperature on external conditions. It was about this time that Galileo invented the thermometer. As observa-

tions now increased, numerous speculations as to the source of animal heat arose. It must be connected with the movement of the blood, for how otherwise could be explained the cooling of a corpse or of a limb deprived of its circulation? The heat might arise in the blood alone or have its origin in the heart, and only be distributed by the blood stream.

It was well known that heat arose during fermentation and by the contact of acid and base; animal heat was, therefore, considered to arise by some similar process or processes taking place in the blood. The former opinion was held by Willis, the anatomist, who, in his treatise "de Ascensione Sanguinis," written about the year 1670, gives the theory that there is in the blood a combustion which depends upon the fermentation excited by the combination of different chemical substances.

Friction was another well-known source of heat, and was the explanation given for animal heat by the celebrated Dutch physiologist, Boerhaave, who lived in the early part of the eighteenth century. He explained the "vital" heat as due to the friction of the particles of blood in the vessels.

A much more correct opinion had already been formed by Magin in 1674. This physiologist, after his experiments in the constitution of air and its relation to the heat of combustion, extended the analogy of combustion to animal heat. He held that the function of the lungs was not to cool the blood, but to enable that fluid to absorb the nitro-aërial spirit (oxygen) of the air, and so generate heat.

Black, in 1757, discovered that carbonic acid gas, which is formed by the burning of fuel, is also given off by the lungs; he thus showed a strict connection between the processes of combustion and respiration; he supported the theory that the heat arising from the union of oxygen and carbon was the real "vital" heat.

A few years later Lavoisier introduced a theory exactly similar to Black's, but how far it was dependent on or independent of Black's work is not known.

A serious objection was quickly raised to Black and Lavoisier's theory; it was shown that if the heat were produced in the lungs, the temperature of that organ would be incompatible with its life. To remove this objection, Crawford in 1781 proposed his ingenious theory, which he had based on experiments upon the specific heats of venous and arterial blood. His experiments appeared to show that the specific heat of arterial was considerably greater than that of venous blood; the combination, therefore, of the blood with the oxygen of the air in the lungs gave rise to the liberation of only a small amount of heat in the lungs; the blood here was arterial. When, however, the blood reached the systemic capillaries, it became venous, and the latent heat was rendered sensible. This was a very "materialistic" explanation of "vital" heat, and was, in certain respects, a distinct advance towards truth.

It is interesting that Crawford and Lavoisier were the first to make experiments on the amount of heat given off by animals, and even to compare the oxidation, as represented chiefly by the carbonic acid, with the amount of heat produced. They showed that the oxidation could produce the necessary heat.

John Davy in 1815 showed by more exact experiments that there was no perceptible difference in the specific heat of arterial and of venous blood, and therefore Crawford's theory must fall to the ground. This, however, by no means affected the doctrine of Lavoisier, that the oxidation in the animal body was the cause of the heat. In fact, Liebig not many years later, by properly interpreting the calorimetric experiments of Daling and Despretz, placed Lavoisier's doctrine on such a firm basis that it became strong evidence for the Law of the Conservation of Energy.

In the last fifty years the labours of Helmholtz, Ludwig and Pflüger have proved that the heat is formed in the tissues, that heat is produced by the activity of muscles and glands. Bernard also, by showing how variations of the circulation of the bood could be effected by the nervous system, threw a great light upon the process of heat regulation. The formation of heat varied in different parts, but the temperature was equalised and regulated by the continual blood-stream ; the circulation of the blood, being under the control of the nervous system, could, when necessary, send more blood to the surface, and so increase the loss of heat, at another time could husband the warmth by diminishing the blood-supply to the skin.

Such have been the chief lines along which the knowledge of the causes of animal temperature has advanced. The doctrine of " vital heat," propounded by the vitalistic school, has fallen before the advance of materialism ; " vital heat " is shown to be similar to heat arising from physical and chemical processes.

Numerous observations upon animal temperature have now accumulated. Of these the most interesting, without doubt, lies in the difference which has been found to exist between the lower and the higher animals. Those animals which are high in the scale of evolution, such as birds and mammals, have a high temperature, and this has a mean value (about 37° C. or 98° F. in man) which is independent of the temperature of the surrounding air. The lower animals on the contrary, such as molluscs, fishes, amphibians and reptiles, have a temperature generally only slightly above that of their surroundings. This difference between the two classes is expressed by the terms " warm-blooded " and " cold-blooded " animals.

The warm-blooded animal has in correspondence with its high temperature a very energetic oxidation within its tissues, and this must, *cæteris paribus*, be more vigorous in the winter than in the summer. The cold-blooded animals, on the contrary, have a much smaller oxidation, and one which is so lessened by cold that in winter

these animals find their energy so reduced that they pass the time in sleep—they hibernate.

The higher animals have been classed as the warm-blooded, the lower as the cold-blooded. This classification is, however, not absolutely exact. Indeed mammals are found, such as the hedgehog, bear, and dormouse, which are in an intermediate position, since, in spite of their high temperature in warm weather, their temperature falls in winter, they become inactive and hibernate, the oxidation of their tissues is reduced to a minimum; on the other side, there are bees, animals of a low order, which have and maintain a higher temperature than most cold-blooded animals, and are not reduced to spend the winter in slumber.

In order that an animal may have under very varying conditions a constant temperature, it must possess some regulating mechanism. There are two means by which regulation can be obtained, variation in the amount of heat produced and variation in the loss of heat. Both methods are found in use. When a white mouse, for example, is removed from cold to warm surroundings, it regulates its temperature by increasing the loss of heat from its body, it sweats and exposes to the air as large a surface of its flushed skin as possible; at the same time, its heat-production is lessened, it is less active, it gives off less carbonic acid, there is less oxidation in its tissues. Let the same animal be now returned to a cold chamber. At once its ears and face become pale, showing that the vessels of the skin contract, and prevent loss of heat by the blood passing to the skin; when not in active movement the animal huddles up together, ruffles its fur, and makes it as bad a conductor of heat as possible; it also regulates the heat production, as shown by involuntary shivers or active voluntary movements, it gives off more carbonic acid, there is increased oxidation in its tissues.

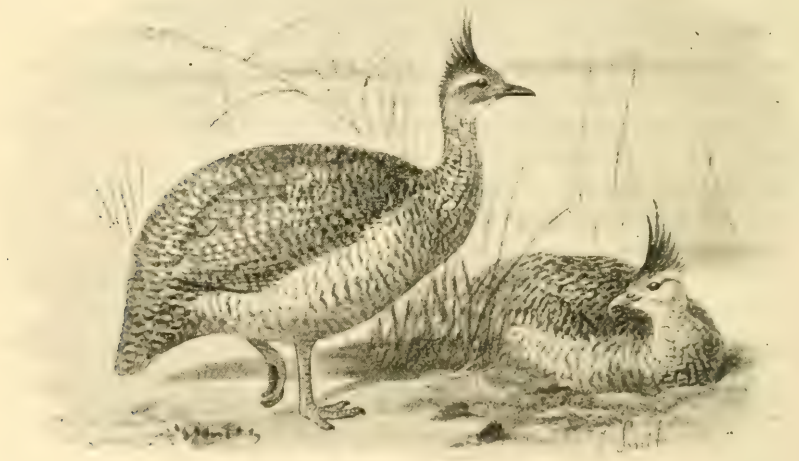
These regulating mechanisms appear to be under the control of certain parts of the nervous system. The exact anatomical seat is unknown, nor are known the anatomical differences which separate the cold-blooded from the warm-blooded animal, and which would explain the condition of the intermediate forms, such as the hedgehog, the dormouse, bees. It has been shown that by the action of certain nervous poisons, and by intense cold, warm-blooded animals may be made to pass into a condition somewhat resembling that of the cold-blooded animals. Ignorance now reigns here, but not so absolutely for ever.

M. S. Pembrey.

SOME NEW BOOKS.

IDLE DAYS IN PATAGONIA. By W. H. Hudson. 8vo. Pp. viii. and 256. Illustrated. London: Chapman & Hall, 1893. Price 14s.

IF every sojourner in remote regions of the world occupied his "idle days" as profitably as has Mr. Hudson, our information regarding the animal life and its inanimate surroundings of the whole globe would be far less incomplete than it unfortunately is at present. We are, indeed, glad to welcome another volume by this author, written in the same charming and lucid style as his "Naturalist in La Plata," and embellished by illustrations executed in the same beautiful style. The present work is, however, far less exclusively devoted to the Natural History of the country it describes



CRESTED TINAMOU *(Calodromas.)*

than was its predecessor, but for a vivid description of the mode of life in Patagonia, and the nature of the country, it can scarcely be surpassed. As with "La Plata," most of the chapters in the work before us have already seen the light in the form of articles in magazines and other serials, and this reproduction has given the author the opportunity of revising and amplifying his descriptions and maturing his opinions.

Confining our attention to those parts of the book devoted to the Natural History of Patagonia, we note that the author gives us excellent illustrations, accompanied with observations on their

mode of life, of the Black Vulture, the Upland Goose, and the Crested Tinamou, as well as of several of the smaller birds. Perhaps the most generally interesting chapter in the whole book is that on "Bird-music," in the course of which the author points out how impossible it is to convey any adequate idea of the songs of birds by mere description. He illustrates this by mentioning that before he had ever visited England he was anxious to obtain some idea of the nature of the melody of the British songsters, but that all his efforts were in vain. And the description of his delight at first hearing the music of our English fields and groves ought to remind us all how little we really appreciate, as a rule, the natural charms of our own land. Possibly some further development of the phonograph may be the ultimate means by which "Bird-music" may be rendered comprehensible to those who cannot hear it at first hand, as we greatly doubt if this can ever be effected by any merely graphic method.

Of scarcely less interest is the author's description of the ways of the leaf-bearing ants, on pp. 140–141; while those whose special study is Mammals will not fail to notice the observations on the burrowing Tuco-tuco (*Ctenomys*) and the Mara (*Dolichotis*). As the author well remarks, it is very curious to find in a burrowing animal the eyes of so large a size as they are in the former of these two rodents.

In commending this book to the attention of our readers, we may express a hope that it will ere long be followed by others from the same ready pen. R. L.

The Great Sea-Serpent: An Historical and Critical Treatise. With the reports of 187 appearances (including those of the appendix), the suppositions and suggestions of scientific and non-scientific persons, and the Author's conclusions. With 82 illustrations. By A. C. Oudemans, Jzn. Published by the Author, October, 1892. London: Luyac & Co.

Of this work we give two reviews, the one by a naturalist, the other by a literary contributor.

I.

The author in his preface compares his work with that of Chladni on meteoric stones. Chladni, he says, opened the eyes of unbelievers by collecting and comparing all accounts of meteoric stones up to the nineteenth century. Meteoric stones were again found and were proved to be quite different from terrestrial stones. Unfortunately for the argument, remarkably few sea-serpents have been caught, and those few have proved to be not at all different from well-known objects.

We confess to have found Oudemans' book exceedingly dul reading. 379 pages are devoted to genuine or invented accounts of various appearances; 110 to explanations hitherto given. No doubt it is useful to have the literature of the subject compiled, but the author might have contented himself with a much greater compression of the interminable newspaper discussions, evidence on oath of sailors and fishermen, and so forth. We do not see that he advances at all beyond Mr. Hoyle's bright and short account of the sea-serpent in the "Encyclopædia Britannica." A number of the records are pure myths; some others are due to mistaken observations of floating sea-weed; porpoises swimming in a row; basking sharks, and so forth. The persistent records from the Norwegian coasts are most probably explained by the existence of gigantic cuttle-fish; and there remains

a residuum which quite possibly may be explained by the discovery of some new animal. Oudemans' own theory is given with a considerable amount of confidence, in some 50 pages towards the end of the book, and on page 516 an ideal sketch of the animal is produced. It is a Pinnipede to be called *Megophias megophias* (Raf.), Oud.; and a phylogenetic table of its relations with other Pinnipedes, living and extinct, is given. It is very possible that a large Pinnipede may exist, but, on carefully going through the characters suggested for it by this author, it is difficult to see that he has been guided in his selection from reports by any sounder principle than relying on what appeared to suit his hypothesis, and rejecting or explaining away inconvenient ones. Further notice of this book I leave to a literary contributor.

P. C. M.

II.

The attitude of the nineteenth century, social as scientific, towards the unknown may be summed up by the remark made by a lady member of the upper ten concerning those less happily situated in the social scale—"I don't know them: they don't exist." In the face of this attitude Dr. Oudemans has presented the public with a treatise of no less than 592 pages on our old friend the sea-serpent. We are warned on the title-page that the little volume contains reports of 187 appearances, "including those of the appendix," but we are not told what the appendix of the serpent is, and there are no special reports on appearances of his tail. The author much deplores the fact that his illustrations are not due to the unlying kodac. Nevertheless, the reader does not lose by this little omission in the baggage of observant travellers. It has been the means of providing portraits of the sea-serpent such as we feel assured no camera yet invented could have produced. The most impressive of these is one compiled by Messrs. Renard, *père et fils*, who combinedly took observations one moonlight night during a voyage on the high seas in 1881. The result is the portrayal of a dragon worthy the sublimest efforts of Sir Augustus Harris. We fancy even our own patron saint would have quailed before it, unless, indeed, following the advice given by Dr. Oudemans, he had armed himself with "explosive balls and harpoons loaden with nitro-glycerine." This particular specimen possessed not only teeth, "sharp, enormous, and white," but a phosphorescent tongue and an eye that looked backwards. Probably that eye also winked, for Dr. Oudemans includes both description and drawing under the heading of "Cheats and Hoaxes"—a section sternly dealt with at the beginning of the book. However, in the next chapter, entitled "Would-be Sea-Serpents," the illustrations are not to be outdone, in spirit and conception, by mere cheats and hoaxes, and in fig. 12, *Lineus longissimus*, Sow., we find something really worth looking at. Chapter iv. consists of reports and papers on the appearances of sea-serpents in various parts of the world. These are mostly compiled by naval officers, and a few—a very few—scientists. The Church, however, is not unrepresented, and we would not like to call in question the evidence of the Archbishop of Upsala, writing in 1555; but surely the Archbishop's illustration is the gem of the collection. The sea-serpent is depicted in the act of swallowing a sailor, and we can only charitably suggest that the original sketch was intended as a representation of Jonah's unfortunate experience. Among so much documentary evidence, it is difficult to discriminate critically. Even Milton is pressed into the service, quotations being given in "Reports

and Papers" from *Paradise Lost*, where the great leviathan is mentioned as being the special dread of the Norwegians (the coast of Norway being the favourite haunt of the monster according to the most reliable eye-witnesses). But why is Coleridge left out?

> "Beyond the shadow of the ship,
> I watched the water snakes.
> They moved in tracks of shining white,
> And when they reared the elfish light
> Fell off in hoary flakes."

Later on, we come upon another picture of a sailor being cut off in his prime by the wily monster, and we pass on with considerable relief to Dr. Oudemans' "Conclusions," where, under the headings of "Harmlessness, Timidity, and Playfulness," we learn that the great sea-serpent is, after all, a mild creature, a conviction probably deduced from the fact that, if the great sea snake has ever shown his animosity to the unwary invaders of his haunts, no victim has, so far, survived to tell the tale.

It is sad, however, to find that Dr. Oudemans' great sea-serpent is, after all, only a monster sea-lion, and we don't know of his having done anything to justify Dr. Oudemans' calling him *Megophias megophias* (Raf.), Oud.

L. B. P.

HANDBUCH DER PALÆONTOLOGIE.—Palæozoologie, vol. iv., pt. i. Edited by K. A. Zittel. 8vo. Pp. 304. Illustrated. Munich and Leipsic: R. Oldenbourg, 1892.

WE are glad to congratulate Professor Von Zittel on the untiring energy which has enabled him to get well into the Mammals in the present fasciculus of this magnificent and invaluable work. It need hardly be mentioned that the whole of the Vertebrate portion of the work has been written by the Editor himself; and the present fasciculus is fully equal to its predecessors in the care and attention

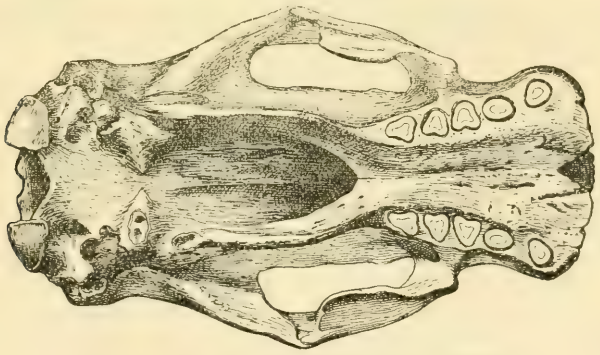


FIG. 1.—Palatal view of the skull of *Mylodon*.

which has been bestowed upon it, and in the excellence of the illustrations. Three of the latter we are enabled to reproduce as samples. We are especially glad to notice that the author has devoted particular attention to the recent important discoveries which have been made among the fossil mammals of South America, and that a host of new forms have been assigned to their proper serial positions. This is the more creditable to the author, seeing that owing to unfortunate circumstances the vertebrate palæontology of that country

has been involved in an intricate labyrinth of confusion, from which it can only with much labour and care be freed. We are persuaded, however, that in accepting in the main the conclusions of Señor Ameghino, the author has been well advised; and we are glad to see the newly-discovered Argentine Tertiary Marsupials placed next the living Australian Thylacine.

Almost the only portion of the work that we do not like is the classification which the author has seen fit to adopt. In place of dividing Mammals into the three primary groups of Ornithodelphians (Monotremes), Didelphians (Marsupials), and Placentals, Professor Zittel takes only two primary groups, namely, Eplacentals and Placentals; the former including the Monotremes and Marsupials, together with the extinct Allotheria, or Multituberculata. Now, we are fully prepared to admit that there is much to be said in favour of a binary instead of a ternary subdivision of the Mammalian class. If, however, such binary scheme of classification be thought advisable, we have no hesitation in saying that the Monotremes (together with the Multituberculata) should form one subclass, while the Marsupials should be brigaded with the Placentals in the second, and that there is no sort of justification for the scheme followed by our author.

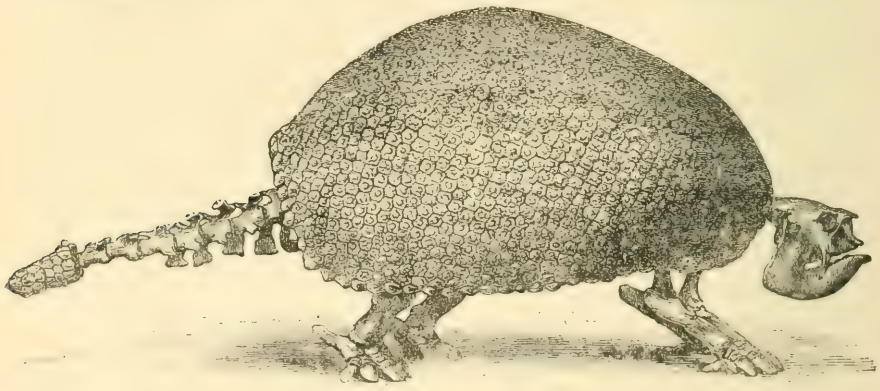


FIG. 2.—Restored skeleton of *Glyptodon*, with the tail incomplete.

The present fasciculus includes the Monotremes, Multituberculata, Marsupials, Edentates, Cetaceans, Sirenians, and the Condylarthrous and Perissodactylate sections of the Ungulates.

A large space is devoted to the Mesozoic Mammals, among which the Allotheria (Multituberculata) are assigned a rank equivalent to that of the Monotremes, while all the other forms are included (and we believe rightly) among the Marsupials, some of the Cretaceous types being even placed in an existing family. There may be some justification for making the Banded Anteater (*Myrmecobius*) the representative of a distinct family, but there is surely none for placing the Peramelidæ between that family and the Dasyuridæ. Then, again, we must take exception to making the Rat-kangaroos a distinct family (to which, by the way, a wrong name is given) separated by *Thylacoleo* and the Phalangers from the Kangaroos (Macropodidæ).

In the Edentates, special interest attaches to the excellent restorations of the skeletons of the Glyptodonts, one of which is herewith given. We are, however, persuaded that the division of the Ground-Sloths into several distinct families is not justifiable. We notice that the author has seen no reason to follow certain new views

which are supposed to justify the removal of the extinct Zeuglodonts from the Whales.

Beyond the circumstance that a large number of the newly-discovered South American forms are described and figured, the only point in the part devoted to the Ungulates to which we need direct attention is in regard to the classification of the Perissodactyles. Following recent American views, the author adopts a phylogenetic classification, and includes in the same family such widely-different forms as the modern *Equus* and the Eocene *Hyracotherium*. On the other hand, *Systemodon*, which is very close to the latter, is placed in the Tapiridæ. That these two Eocene forms may be the ancestral types of the two families to which they are respectively referred, we are not going to dispute for a moment. We do say, however, that to include them in such families, and to separate them from one another, is, to our mind, only confusing classification. Moreover, if we are to have a phylogenetic classification, the author does not go far enough.

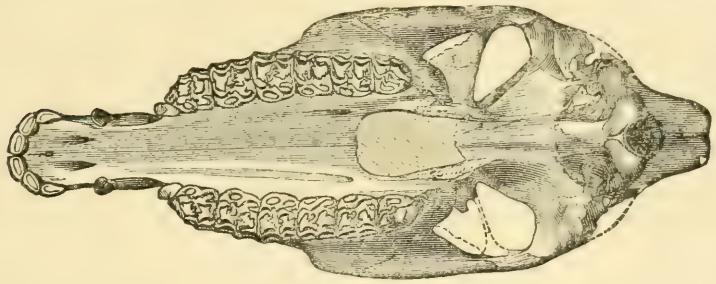


FIG. 3.—Palatal aspect of the skull of *Hipparion*.

Surely, seeing that the Condylarthra are admittedly the ancestors of the Perissodactyla and Artiodactyla, there is just the same justification for putting all three in a single group, as there is for refusing to admit that there may be an Eocene family (Lophiodontidæ) which shall include the ancestors of both the modern Equidæ and the Tapiridæ. As an example of the illustrations among the Equidæ, we reproduce one of the skulls of *Hipparion*.

That the work (however much we may be disposed to differ from it as regards points of detail) when complete will be of inestimable value to the palæontologist and zoologist, we have already had occasion to say elsewhere, and our only regret is that the limited cult of the former science in this country forbids the publication of one on the same lines in England. R. L.

ÉLÉMENTS D'ANATOMIE COMPARÉE. By Remy Perrier. Pp. 1,000. With Illustrations. Paris: J. B. Baillière et Fils, 1893. Price 20 francs.

ZOOLOGICAL text-books seem at the present time to be multiplying at a rate altogether out of proportion to the numbers of the students for whom they are intended. This is not only the case in this country, but also on the Continent. The commencing student of zoology has so many works to choose from (we cannot, however, justly say that there is an *embarras de richesses*) that he probably ends by taking one at random. It would be much better if the elementary student would refrain altogether from purchasing text-books, and trust himself to the

professor whose lectures he attends. However, if he must buy a text-book, then let him consider two standards by which, in our opinion, a book of this kind should be judged. A book intended for elementary students ought to be of moderate bulk, and it should be thoroughly up to date. In these days of examination, the very newest discoveries are apt to be inquired about by examiners. The book should be rendered fairly portable through judicious condensation, not so pronounced, however, as to produce obscurity. The writer of a text-book should remember that human flesh is weak, and also that time is short. M. Perrier's book is not, judged by these standards, altogether ideal; still, we gladly admit that it has its good points. Possibly, too, the French student is differently placed with regard to examinations. The author might, however, if he had taken thought, have refrained from adding an inch or two to the stature of the book; this would have been a distinct gain, for we have seldom handled so awkwardly shaped a volume. If there is a second edition it is to be hoped that a fission into two equally-sized halves will take place.

A feature of M. Perrier's book is the introduction of coloured plates, illustrating, of course, the circulatory system of various animals. The illustrations are not generally very first-rate, and the same remarks apply to the woodcuts and "process" blocks by which the text is really very lavishly illustrated. What is wanting in quality is certainly made up in quantity. Furthermore, the author pays more attention to papers published outside France than is sometimes the case with his countrymen. Altogether, we are favourably impressed with M. Perrier's book.

TEXT-BOOK OF THE EMBRYOLOGY OF MAN AND MAMMALS. By Dr. Oscar Hertwig. Translated from the third German edition by Edward L. Mark, Ph.D., Hersey Professor of Anatomy in Harvard University. Pp. 670, with 339 figures in the text and 2 lithographic plates. London: Swan Sonnenschein & Co., 1892.

ALMOST as much as in the case of Balfour's great treatise on comparative embryology, this book is the result of the author's original investigation. The two books have very different purposes. Balfour was practically inventing a new science, and he endeavoured, so far as could be done, to go over the whole animal kingdom, and set forth, in orderly and systematic exposition, the facts and the principles of Comparative Embryology. The very result of the enormous stimulus to research given by his treatise is, that the treatise is now so largely behind our knowledge that it is rather a landmark in history than a text-book for beginners. Dr. Oscar Hertwig has written his book specially from the point of view of the needs of those studying human anatomy. The facts and principles of comparative embryology are used chiefly as a guide to the ontogeny of higher forms. But the student of medicine or morphology who masters this volume will have a large knowledge of comparative embryology, and will have the unity of the animal kingdom strongly impressed on him. It would be difficult to think of anyone from whom a book of this kind would be more valuable, for a large part of the extension of our knowledge has been due to the studies of Oscar Hertwig and his brother on the Cell and the Coelom-Theory.

Those who have been unable to follow recent developments of embryology closely, will be most interested by the chapters on the middle layer. Not long ago it was assumed that the mesoblast was a

germinal layer of equal value with epiblast and hypoblast. True, it appeared later in development, and its mode of origin was in dispute. Here there will be found a different conception. Hertwig divides Metazoa into animals with two germ layers and animals with four germ layers. In the latter, the two middle layers are both derived as definite organs from the inner layer; the one is the median notochord; the other, the paired sacs of the body cavity. In Amphioxus alone, this condition is perfectly definite; but the recent discovery[1] of segmental ducts, and nephridia in Amphioxus certainly will increase the content of morphologists in regarding Amphioxus as a representative of the ancestral Chordata. In higher forms, the presence, actual or ancestral, of masses of food-yolk has so compressed the pouch-like evaginations from the primitive gut that they arise as solid outgrowths.

Hertwig keeps by themselves these four primitive layers as "Epithelia," which serve for the limitation of the surfaces of the body. At the close of segmentation only one layer is present—the epithelium of the blastula. From this the other layers arise by infoldings and outfoldings. But there is another element of development. In the Cœlenterates and Echinoderms, when the two primary layers are established, there is found between them a supporting gelatinous layer. There wander into this from the primary layers cells most often starting from the point where these layers pass into each other. These immigrant cells lose their epithelial character and spread out as branched cells like connective tissue corpuscles. This factor in development is called by Hertwig the "mesenchyme," and he finds it in the development of all higher forms as the origin of the connective tissues and the blood.

Agreeably to the intention of the book as a text-book, this theory is laid down crisply and definitely, and it certainly is an extremely important advance on current ideas, although, for reasons that will readily occur to specialists, it may well be that the two middle layers and the mesenchyme do not exhaust that complex telescoping of forgotten stages in the development of organs we call mesoblast.

For the printing, illustrations, index, and references to literature, the highest praise is due. The translator has done his work well, but he is not to be congratulated on his translation of the word "anlage" —a translation on which he plumes himself so much in the preface. "Fundament" is a clumsy word, and both the noun and adjective have meanings already in possession. As an English equivalent, "forecast" is the best word, and for an adjective "incipient" is very convenient.

P. C. M.

Die Zelle und die Gewebe. [The Cell and Tissue.] By Dr. Oscar Hertwig. Pp. 300, with illustrations. Jena: Gustav Fischer, 1892. Price 8 marks.

A book treating of the structure of cells, written by Dr. Hertwig, and published by Herr Fischer, of Jena, is sure to be excellent in matter and in style. The volume before us is, on account of its very excellence, difficult to use as a text for a short discourse in this review. Merely to praise is apt to produce rather laborious reading. The only proper alternative would be to give a general account of the book; but this would occupy a little too much room; we shall therefore

[1] Since the publication of this edition.

content ourselves with noting a few points. The subject of which Professor Hertwig treats is one which the Germans have made particularly their own; it is true that eminent persons, not Germans, such as Professor Edouard van Beneden, have investigated these matters, but the bulk of the work has been done in Germany.

Professor Hertwig is, of course, a zoologist, but he has by no means neglected the botanical side of his subject; indeed, the work would be comparatively valueless if he had. It is one of the most remarkable generalisations of Biological science that animal and vegetable cells are identical in all important characters. This was first realised when the identity of their protoplasm was proved by Von Mohl; and the discovery of the nucleus, first due to our countryman, Robert Brown, led the way towards proving another fundamental resemblance between the structural units of the plant and the animal. Finally, the recent discoveries that the phenomena of the dividing nucleus termed karyokinesis were not peculiar to the animal cell, but also characterised the vegetable cell, left nothing wanting to prove the close similarity of the two. There are still remaining certain cells in which a nucleus has not been discovered; on the other hand, the balance is restored by the existence of other cells which have more than a fair share of nuclei. As to the former class, it is doubtful whether a nucleus is ever really wanting. The mammalian red blood corpuscle will at once occur as an instance of such a cell; but, as Professor Hertwig points out, these bodies are not the equivalents of cells at all. Some of the simpler amœboid organisms appear to be without a nucleus, and it was suggested that in such cases the organism was really a free nucleus, with little or no protoplasm. This suggestion was made, we believe, by Dr. Will; Dr. Hertwig alludes to the matter, but does not quote Dr. Will. The Bacteria are organisms whose real nature has been disputed; some have been unwilling to allow to these "plants" the rank of a cell. Bütschli, however, found deeply staining bodies in Bacteria to which he assigned the value of a nucleus. Dr. Hertwig does not refer in this connection —as he might have done—to Mr. Harold Wager's investigations into the nuclei of Bacteria, communicated to the British Association at Cardiff.

An important part of this work deals with karyokinesis. To the general account is appended a brief historical sketch of the matter, and a discussion of the more debatable points, and among these the origin of the centrosoma is the principal one. After giving the arguments in favour of its nuclear derivation, the author ends with the opinion that the time is not ripe for a definite settlement of the question. In the current number of the *Quarterly Journal of Microscopical Science* is a valuable paper upon these matters.

An interesting section of the book deals with the position of the nucleus in the cell—that is, its relation to growth, deposition of "formed substances," &c. The nucleus is all-important; a cell, in fact, is a territory governed by a despot, the nucleus presiding over all the activities of the protoplasm. It was pointed out a few years ago that the yolk in the ovum was first formed in the neighbourhood of the nucleus, a discovery that entirely does away with a view at one time current that the yolk, or, at any rate, a good deal of it, was not of home manufacture, but fabricated outside the ovum, and conveyed to it through the pores in the egg-membrane. Dr. Hertwig quotes and illustrates from the observations of Haberlandt upon the relation of the nucleus to thickenings in the cell-wall of vegetable tissues, and to the growth in length of cells; when these circumstances are

occurring, the nucleus is always apparently to be found at the seat of action. Finally, there are the important studies of Gruber and others upon the artificial breaking-up of cells; in such experiments it has been found that a fragment of the original cell in which the nucleus is left can be regenerated anew, while the remaining pieces live for a time, but eventually decay. The book is, in fact, a very complete and impartial summary of existing knowledge upon one of the most important and fascinating branches of Biology; but it is, of course, written by a specialist for people who are acquainted with, at least, the outline of the subject. Nevertheless, in the proper sense of the word, it ought unquestionably to be a most popular work.

THE GAME BIRDS AND WILD FOWL OF THE BRITISH ISLANDS: Being a Handbook for the Naturalist and Sportsman. By C. Dixon. 8vo. Pp. xvi. and 468. Illustrated. London: Chapman & Hall, 1893. Price 18s.

THE stream of books on British Birds seems to be an endless one, but, nevertheless, there is doubtless an opening for the present volume, which deals with all those groups of special interest to the sportsman, namely, the game birds properly so-called, the pigeons, rails, plovers and their kin, as well as the water-fowl. In his preface, Mr. Dixon—whose name is already well-known through several popular works on ornithology—is careful to observe that he is largely indebted to the writings of others for much of the material on which his work is based, although his own ornithological studies have enabled him to add a considerable amount of original matter connected with the habits of the birds described. Since, we believe, there is no other work of this kind covering precisely the ground occupied by the volume before us, Mr. Dixon's handy and well-illustrated manual ought to be acceptable to all sportsmen who take interest in the natural history and habits of their quarry.

We cordially agree with the author's prefatory remarks concerning the subject of bird classification, and the haste with which almost every aspirant to ornithological knowledge propounds some startling new scheme, destined to revolutionise the science—until superseded by the next attempt; and we think he has done well in keeping more or less closely to one of the older arrangements. In attaching the termination "*formes*" to each one of the old ordinal names, such as "Anseres," we are of opinion, however, that he is ill-advised; the only possible advantage of such a cumbersome affix being in cases where the terms are employed in the sense of subclasses, as, for instance, when Mr. Sharpe uses the name "Anseriformes" to include the order "Anseres" and several others of the old ordinal groups. In placing the Sand-Grouse under the "Galliformes," rather than with the "Columbiformes," the author has no sort of justification, as the osteology of these birds is alone amply sufficient to show that their affinity is much closer with the former than it is with the latter group. Neither are we yet convinced of the propriety of removing the Bustards from the vicinity of the Cranes, and placing them with the Plovers.

The mention of Cranes reminds us that under the heading of the Demoiselle Crane, the author states that the time during which this bird may be taken is "August 1st to March 1st, otherwise by authority of owner or occupier of land." Now, seeing that there is but one solitary record of the occurrence of this species in the British

Islands, it strikes us that the introduction of the statement in question is a trifle superfluous. Indeed, in a book intended primarily for the sportsman, we should have thought even any mention of this particular species of Crane might have been perfectly well omitted.

These are, however, but unimportant blemishes in a work which we can thoroughly recommend to the attention of the class of readers for whom it is specially designed, as being both readable and, so far as we can see, at the same time accurate and up to date.

R. L.

THE HEMIPTERA HETEROPTERA OF THE BRITISH ISLANDS. By Edward Saunders, F.L.S. 8vo. Pp. viii. and 350, with a structural plate. London: L. Reeve and Co., 1892. Price 14s. (Large paper edition with 31 coloured plates, price 48s.)

IT is with pleasure that we chronicle the appearance of this work, which is uniform with Canon Fowler's "Coleoptera of the British Islands," lately issued by the same publishers. Since the appearance of the Ray Society Monograph of the British Hemiptera by Messrs. Douglas and Scott in 1865, and Mr. Saunders' own excellent synopsis of the order in 1876, numerous species of bugs have been added to our fauna, and inevitable changes and re-changes in nomenclature have been made. The classification and synonymy of the British Hemiptera are now brought thoroughly up to date, and it is to be hoped that the book will incite many naturalists to the study of these interesting insects, which, though much neglected in comparison with moths and beetles, well repay the student for his work, and in some respects offer him more opportunity for discovery than the more popular groups.

Only 12 pages are devoted to an account of the affinities, anatomy, and development of the Hemiptera, and the methods of their capture and preservation. It is to be regretted that this portion of the work has not been carried to a greater length; collectors, even now, too often think that to give a specimen a name is the only end of their study; and attention might well be directed to some of the morphological problems towards whose solution the commonest bug may contribute material. Short though it is, however, this introductory portion is clear and reliable, and as far as external structure is concerned, well illustrated by the plate.

The book, however, is issued as a systematic work and, as such, it is excellent. In his arrangement the author follows Puton; entomologists familiar with the sequence of the families in Douglas and Scott will notice that the Hydrometridæ and Hebridæ are brought forward to the middle of the series between the Aradidæ and Reduviidæ, while the Capsidæ are relegated to the end. There are synoptical tables of the genera and species which must have cost the author great trouble, and which, in conjunction with his full and accurate descriptions, should safely guide the student to correct determinations. Those who can afford the large edition will find, in some cases, the coloured figures a "royal road" to identification; but among the more critical groups, such plates are of doubtful utility. The Hemiptera are classified in the main not by colour, but by structure; and outline figures of the distinctive parts in nearly allied genera and species would be of more value than the coloured plates, good as these are. In the table of the genera of the Capsidæ,

for example, which extends over six pages, and in which the differences are often relative, a few good outlines would be invaluable to the beginner. We could also wish to see figures of the immature stages of some of the Hemiptera.

The descriptions are occasionally relieved by accounts of habits, and a full list of the known British localities for each species is given. In a group so comparatively little worked, these localities are at present, probably, as much a guide to the distribution of hemipterists as to that of Hemiptera; the southern counties of England, and Norfolk, however, seem to have been so well searched that species absent from them may be presumed to have a northern or western range in the British Islands. It is a pity that Mr. Saunders has not given at least the outlines of the distribution of each species abroad. A knowledge of this must go hand in hand with that of the British range of an animal, to enable the student to form conclusions as to the relative age of allied forms, and the time and method of the introduction of the various elements of our fauna. Mr. Saunders, in his hints on collection and preservation, lays stress on the necessity for recording localities. It is to be hoped that collectors will all take his advice. Entomologists now, happily, recognise that the value of a specimen is vastly increased when its locality is known; the patient accumulation of such records may furnish invaluable material for future workers at animal distribution and its allied problems.

Altogether Mr. Saunders' work must be considered a most valuable addition to the literature of the British fauna.

THE FAUNA OF BRITISH INDIA, INCLUDING CEYLON AND BURMA: MOTHS. Vol. I. By G. F. Hampson. 8vo. Pp. xxiv. and 527, with 333 woodcuts. London: Taylor & Francis, 1892.

THIS is the first volume on invertebrate animals which has appeared in the excellent series of Indian faunistic works edited by Dr. W. T. Blanford. It contains synopses of the families of moths, and of the Indian genera and species of most of the families included under the terms Sphinges and Bombyces. Mr. Hampson, however, rejects all tribal divisions of the Lepidoptera except the primary separation into butterflies (Rhopalocera) and moths (Heterocera), and he appears to consider the groups of Noctuids, Geometers, Pyralids, &c., as of only family, and not tribal, value.

After a short introduction on the external anatomy of the Lepidoptera, there is a "tree" showing the supposed relationships between the families of moths, followed by a table in which these families are differentiated, mostly by the neuration of the wings. Mr. Hampson has followed, in the main, the classification propounded by Snellen in his "Vlinders van Nederland." British lepidopterists will notice that here, as in Mr. Kirby's recent "Catalogue of Lepidoptera Heterocera," the Sphingidæ are deposed from their place at the head of the moths, and placed next the Notodontidæ. In Mr. Hampson's genetic tree the Saturniidæ appear at the head of one high branch, while the Epicopiidæ, Uraniidæ, and Geometridæ figure at the summit of the other. The Micropterygidæ and Hepialidæ are considered the lowest families, though removed from the main stem, on which the Tineidæ take the lowest place.

The descriptions of the genera and species are illustrated by cuts, the pattern of the wings being generally shown on one side, and their neuration on the other; details of the antennæ, palps, &c.,

are also often given. Mr. Hampson has "lumped" not a few "species"; for example, *Protoparce orientalis*, Butl., is given as a form of the European *P. convolvuli*, L. It is becoming more and more evident that in many groups of Lepidoptera the terms "species" and "variety" must be used according to the taste of the author.

We are promised two more volumes of the work during the next three years, bringing the series down to the end of the Pyralidæ. The remaining families are to be worked out by Lord Walsingham. The volume before us will be gratefully received by students of Indian moths, as it meets a real want, and will prove an excellent guide to future workers.

DOVER COAL BORING: Observations on the Correlation of the Franco-Belgian, Dover, and Somerset Coal-fields. By Francis Brady, C.E. June, 1892. With Reports by M. Victor Watteyne, Mons; James McMurtrie, F.G.S., Radstock; and M. R. Zeiller, Paris. [No place of publication, printer, or date; but Watteyne's Report is dated 16 July, 1892; McMurtrie's, 8 October, 1892; and the section, 19 December, 1892.] Pp. 41. Map (South Wales to Liége), and detailed section of boring.

THIS is the official report on the boring to the westward of the Shakespeare Tunnel, Dover, sunk at the instance of Sir Edward Watkin, on behalf of the Channel Tunnel Company, in search for workable coal seams thought to occur at this point. Although the various Reports differ somewhat, the mysteries surrounding the venture are now dispersed by the detailed section, from which we gather that the following beds were penetrated, commencing at the bottom of a 44 ft. shaft of 9 ft. diameter :—

Depth. Ft.		Thickness. Ft.	in.
91	Lower Grey Chalk	91	0
130	Indurated Chalk Marl	39	0
138	Chloritic Marl	8	0
186	Light-coloured Gault	48	0
256	Dark blue Gault	70	0
259	Sandy Gault	3	0
1113	Various beds representing the Lower Cretaceous and Jurassic	854	0
1136·6	Shale, Sandstone, and Bind	23	6
1140	Coal	2	6
1229	Shales, &c., (with a 6 in. Coal)	88	6
1231	Coal	2	0
1277	Shales, &c.	41	0
1279	Coal	2	0
1311·9	Shales, &c.	25	0
1313	Coal	1	3
1433	Shales, &c.	120	0
1434	Coal	1	0
1456	Shales, &c.	22	0
1458·6	Coal	2	6
1570	Shales, &c.	109	6
1572·3	Coal	2	3
1763·9	Shales, &c.	191	6
1766·6	Coal	2	9
1831	Shales, &c.	64	6
1832·8	Coal	1	8
2038	Shales, &c.	207	0
2039	Coal	1	0
2177·6	Shales, &c.	138	6
2181·6	Coal	4	0

In the report by Mons. Watteyne, which follows the section, we read: "The Coal-measures were reached in beds practically horizontal,

and it was bored in them as deep as 1,940 ft., the horizontal bedding being continuous. In that thickness of 783 ft. seven seams, being more than 1 ft. 6 ins. thick, were found. . . . Two facts are to be quoted about the Dover boring—the *horizontality* and the *regularity* of the beds." But this, it is pointed out, may be due to the boring being just made in the axis of a fold. From analyses made by M. Watteyne, he believes the coal to be of the same quality as the "gras flambant" of the Belgian basin. Under similar conditions of thickness and depth, the average cost of working the Hainaut seams in 1890 was 10 fr. 33 c. (8s. 3d.) per ton.

In Zeiller's report on the fossil plants (which has already appeared in the Comptes Rendus of the Academie des Sciences) the following lists are given:—1,894 ft.: *Mariopteris sphenopteroides* (?), Lesq.; *Neuropteris scheuchzeri*, Hoffm.; *N. varinervis*, Bunb.; *N. tennifolia*, Schl.; *Lepidodendron aculeatum*, Sternb.; a *Cordaicarpus* (?) *congruens*, or *Carpolithes corculum*, Sternb. 1,900 ft.: *N. scheuchzeri*; *N. varinervis*; *N. tennifolia*; *Cyclopteris*; *Calamophyllites goepperti*, Ett.; *Lepidostrobus variabilis*, L. and H., and *Cordaicarpus*. 2,038 ft.: *N. scheuchzeri*; *Lepid. lycopodioides*, Sternb.; *Stigmaria ficoides*, Sternb. M. Zeiller concludes his report with these words:—It may therefore be concluded from the presence of these two species [*N. varinervis* and *N. scheuchzeri*] in the Dover boring, that, as presumed by Mr. Brady, the beds traversed by this boring rightly belong to the upper region of the Middle Coal-measures, and, if one may state it more precisely, that they cannot be either more recent than the beds of Radstock, in Somerset, or more ancient than the deepest beds of the upper zone (*charbons gras et fléxus*) of the Pas de Calais.

Analyses of the Dover Coal (lower seams) gives 83·80 carbon, 4·65 hydrogen, ·97 nitrogen, 3·23 oxygen, with heating power in units of 14,867. Eight workable seams have been met with (a total thickness of 16 ft. 11 in.) between 1,181 and 1,875 ft. from the surface, while the lowest yet met with, 4 ft. thick, and 2,222 ft. from the surface, is, as pointed out by M. Watteyne, well within payable depth, as worked in Belgium. The rest of this interesting pamphlet is taken up with commercial speculations, hopes, and anticipations.

Le Caoutchouc et la Gutta-Percha. By E. Chapel. Pp. 600. Paris: 1892. Price 20 francs.

This work, by the Secretary of the "Chambre Syndicale des Caoutchouc, Gutta-Percha, etc.," gives a complete history of the origin, composition, and manufacture of the industrial products mentioned. The first 80 pages of the book deal with the history of the substances, *i.e.*, the early discoveries and uses, vulcanisation, etc. The second part, pp. 81–295, treats of the botany and geographical distribution of the trees producing the exudation, and appears to collect together much that is valuable both from a botanical and an industrial point of view. Pages 297–340, deal with the physical properties, composition, action of heat and reagents, vulcanisation and its alterations, and the elimination of the sulphur, while the fourth part (pp. 341–519) is devoted to the manufacture and uses. The fifth division of the book is reserved for Gutta-Percha, which is treated in the same systematic way as Caoutchouc; and the sixth and last part deals exclusively with the commercial questions, export and import, custom dues, publications especially concerned with the industry,

and similar matters. There are many illustrations, and the book is interesting not only to the manufacturer but to the general reader.

A Text-Book of Tropical Agriculture. H. A. A. Nicholls, M.D., F.L.S. 8vo. Pp. xxi. and 312, with illustrations. London: Macmillan & Co., 1892. Price 6s.

This book is the outcome of a competition. The Jamaica Government offered a premium for the best Text-book of Tropical Agriculture specially adapted for the use of colleges and higher schools in that colony, and the author having sent in the manuscript of part of the book before us, it was awarded the prize on condition that some chapters were added treating of most of the cultivated tropical plants not noticed in the original. This was done, and in 1891 the Government of Jamaica published the book, of which Messrs. Macmillan have just issued the London edition, of the convenient size, general excellence of production, and with the bright scarlet cover of their other manuals for students.

The work has already passed the test of practical experience in Jamaica, and is now being adopted by the Governments of other colonies.

The author hopes to serve not only scholastic institutions, but peasant proprietors, owners of small estates, and intending settlers in tropical countries, and the fact, as stated in the preface, and evident from the merest glance at the text, that we have here "not a mere compilation, but the record of experience gained by study, observation, and experimental cultivations," should ensure it success.

The book is divided into two parts. Part I.—"Elements of Agriculture"—is a capital introduction to the science, occupying 85 pages, well arranged in thirteen chapters. The first is brief and introductory, the second and third give a concise account of Soils, how they are formed by atmospheric and other action, their constituents, classification, and properties. Chapters 4 and 5, on Plant Life, tell in about 20 pages of the parts of a plant, its nutrition, composition, and reproduction by seed or vegetative processes. A short chapter follows on Climate, "the greater or less degree of heat, light, and moisture," and the influence thereon of elevation, forest, aspect, or soil. Then under manures is considered the restoration of fertility to an exhausted soil, or the improvement of a poor one, and the most important "general" and "special" manures, their preparation, use, and effect, are described.

Chapter 8, "Rotation of Crops," follows, and is, unfortunately, necessarily short, very little attention, as the author remarks, having been given to working out a proper system of rotation in the West Indies, as has been done in Europe and North America. The importance of experiments is urged, and a course of rotation suggested, viz., yams or tanias (the rhizome of *Colocasia esculenta*) the first year, maize the second, sweet potatoes the third, and castor-oil or a similar crop for the fourth. Stress is also laid on the beneficial mechanical effect on the soil, from the necessary frequent turning over and consequent exposure to the chemical action of the atmosphere, while the rotting of roots of former crops make channels in all directions for water and air. Moreover, a proper system of rotation prevents blight and keeps away destructive insects

that confine their depredations to particular plants, as these pests will be starved out in the seasons intervening between the recurrence of their especial crop.

Drainage, Irrigation, and Tillage Operations form the subjects of the next three chapters, the third containing a description of the various instruments and their uses. Then a few words on Pruning, which, it must be remembered, is a scientific operation quite different from hacking or mere reduction of the bulk of a tree, and finally a chapter (13) on Budding and Grafting; this, by the way, with some good illustrations.

Part II.—Agriculture Products—occupies the remainder or rather more than two-thirds of the book. It is a most interesting account of the principal plant-products of our West Indian Colonies, and the mere general reader, as well as the man of science, will find it so, comprising as it does a well-written history of the growth and preparation of such everyday commodities as coffee, cocoa, tea, sugar, spices, drugs, dyes, or tobacco, "most universally used by mankind."

Tropical cereals, fruits, and food-plants like cassava, arrowroot, yams, and others, are also considered. Take, for instance, the chapter on Coffee. The idiosyncrasies of the two cultivated species, both of which hail from Africa, *Coffea arabica*, and *C. liberica* are mentioned, then its propagation in the seed-beds or nurseries is described, and a word is thrown in in favour of bamboo-pots, made by sawing the stem through an inch or two below each node, as strong and inexpensive. Then follow the preparation of the land, holing, planting out, the necessity for shade, weeding, topping, pruning, manuring; a "catch crop" may be reaped with advantage while the shrubs are growing; the vacant ground between the rows being planted with maize, plaintain, sweet potatoes, or other such food-products. A brief account is given of the enemies of the coffee trees and how to meet them; and, finally, the gathering of the crops and the preparation of the berry for market.

A similar plan is followed in the case of other products, and, in a few cases, sketches of the plant or shoot with the flower, fruit, or seed are also given. These sketches might have been more numerous and exhaustive; they are generally small and convey but little information.

Dr. Nicholls has, of course, dealt only with the West Indian products, some of which are, however, universal in the tropics, but the principles inculcated are so general, that the work will be valuable in the Old as well as the New World, and deserves to be widely known.

Le Thé, botanique et culture, falsifications et richesse en Caféine des différentes espèces. By Antoine Biétrie. Small 8vo. Pp. 156. With 27 figures in the text. Paris: Baillière et Fils, 1892. Price 2 francs.

Struck by the ignorance of most of its consumers as to the cultivation and preparation of tea, by the consequences of its immoderate use, and the numerous methods of estimating the active element therein, the author of this little volume was induced to study some of its properties both from a botanical and chemical point of view. His account should especially interest English-speaking people for, from the statistics given on page 11, they are by far the greatest consumers; the Australian heads the list with 2·9 kilogrammes per person, then follow the English and Canadian with 2 and 1·98 respec-

tively, the French are next with only ·9; then the American with ·6; the Dane with ·2; and other European nations with a rapidly decreasing amount down to the Italian, who is presumably satisfied with ·001.

The book is divided into three parts; the first, dealing with "Botany and Culture," begins with a botanical description including a good figure of a shoot, and also sketches of the leaf and parts of the flower. Chapter II. tells of a few unproductive attempts at European cultivation, and gives a very brief account of the culture and the three yearly pickings in China. Then the preparation of green and black tea is described, the difference lying in the treatment of the leaves; the author expresses his belief in the view that the black owes its colour to the fermentation the leaves have undergone, to which is also due the much greater percentage of ammonia salts than in the green, the proportion being as 40 to 13. Anyhow, as we read in chapter IV., tea is a delicious beverage, though liable to be spoilt in the making; accordingly we are told how they make it in China *fide* General Tcheng-Ki-Tong. In Japan they do it a little differently, and in France differently again; the English method, which strikes us as infinitely superior to them all, is not given.

The physiological action includes not only the well-known effect on the nervous system, but also that on the circulation and respiration.

The second part gives a useful account of its adulteration with leaves from other plants. Rough figures of transverse sections illustrate how by presence or absence of certain tissue elements and arrangements we can distinguish the true from the false.

Part III. is chemical; it tells us what tea contains, and how the green and black varieties differ. The latter are usually much richer in the characteristic alkaloid caffein or thein, which M. Biétrie decides to consider as identical; several of the methods of estimating the alkaloid are described. The last part contains short diagnoses, so to speak, of a large number of "teas," with hints for distinguishing them by physical characters.

We quite enjoyed reading the book, though it has the French failing of falling to pieces as soon as opened.

Messrs. Baillière & Sons, of Paris, have just issued a new catalogue of zoological works, referring especially to fishes (and fisheries), reptiles, and batrachians; also a useful catalogue of phanerogamic botany.

We gladly welcome the second volume of Mr. Blake's *Annals of British Geology*, containing abstracts of the books and papers published during 1891. The editor's comments, which were not always gratefully received, are now relegated to footnotes. The abstracts are generally well done, but it seems a pity that so much space is occupied by lists of names taken from British Museum catalogues.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Professor Dr. Antonio Borzi, of Messina, has been appointed ordinary Professor of Botany and Director of the Botanic Gardens at Palermo.

Dr. Fausto Mori, hitherto extra-ordinary Professor of Botany in the University of Sassari, succeeds Professor F. Tornabene as extra-ordinary Professor of Botany in the University of Catania.

Professor Friedr. Oltmanns, Assistant at the Rostock Botanical Institute, has been appointed extra-ordinary Professor of Botany in the Philosophical Faculty at Freiburg-im-Breisgau.

The Chair of Vulcanology at Catania, rendered vacant by the death of Professor Silvestri, has been abolished. We are glad to learn, however, that a new Chair has been founded in the University of Naples, to which Dr. H. J. Johnston Lavis has been appointed.

Mr. W. R. Ogilvie Grant has been promoted to the rank of First-Class Assistant in the Zoological Department of the British Museum. He has just completed the manuscript of the volume of the Ornithological Catalogue relating to the Game Birds.

In accordance with an Imperial decree, issued on February 9, the city of Dorpat (or Derpt) is henceforth to be known as Jurjeff. The name of one of the oldest and most renowned universities in Russia is thus changed. It is merely one more step towards the Russianising of the Baltic province.

The *Botanical Gazette* of January last informs us that the State University of Iowa has sent Professor B. Shimek to Nicaragua "to follow the route of the canal as near as practicable and make a general investigation of the country, its general character (fertility, climate, &c.), its people, its geology, its flora (special attention being paid to the cryptogamic flora), and its fauna." As the Professor is expected back in Iowa City with his collections "not later than April 1, 1893," he must indeed be a rapid worker to render a satisfatory account in all these departments.

The new University of Chicago is making a grand start, and we heartily wish it success. The following is a list of the teaching staff, so far as geology is concerned: T. C. Chamberlin, Head Professor of Geology; R. D. Salisbury, Professor of Geographic Geology; J. P. Iddings, Associate Professor of Petrology; R. A. F.

Penrose, Jr., Associate Professor of Economic Geology; C. R. Van Hise, Non-resident Professor of Pre-cambrian Geology; C. D. Walcott, Non-resident Professor of Palæontologic Geology; W. H. Holmes, Non-resident Professor of Archæologic Geology; George Baur, Assistant Professor of Palæontology (Biological Department); Edmund Jussen, Docent in European Stratigraphy. If other departments of knowledge are represented on anything like the same scale, this will be the most completely equipped University in the world. The geological professors propose to start a monthly magazine, which will be issued under the auspices of the University.

WE much wish that space allowed us to print in full the strong circular-letter of protest addressed by Dr. John Young to Lord Kinnear, as chairman of the Scottish Universities Commission, on the "omission of Geology from the subjects for which it is proposed to endow chairs in Glasgow University." At present the teaching is not the duty of any Professor in the University, and it has been carried on by means of lectures, by Dr. Young himself, under the Honyman-Gillespie Endowment. For twenty-seven years Dr. Young has divided the Natural History chair into two subjects—Zoology and Geology—and he says sorrowfully, "I have given the best years of my life to minimise the evils of a duplicate commission, or, rather, professorship, and it is hard indeed to find that my earnest appeal on behalf of my chair, my emphatic evidence that I cannot keep pace with two sciences—either of which is enough for one man's energy—have evoked no hint of help from a Commission appointed to improve the teaching in the Scottish Universities." The University has, strangely enough, divided the History chair into Civil and Ecclesiastical, but ignores the claims of Geology. Dr. Young concludes:—"I fervently hope that before I demit office, Zoology and Geology may be adequately provided for, and my successor spared the weary task which has been mine for so many years."

IN a new storey recently added to Firth College, Sheffield, Professor Denny is provided with accommodation for the work of the Biological department to the extent of a laboratory and museum combined, and a lecture theatre communicating with it. The dissecting benches at present available for work seat about thirty students. It is satisfactory to find the number of students steadily increasing every year. In the development of a teaching museum Professor Denny has so far devoted the very limited fund at his disposal to the acquisition of osteological preparations and embryological models, which already fill the museum cases provided. The rooms are fitted with electric light throughout. Hitherto the biological work of the College has been carried on in the museum of the School of Medicine.

WE notice that the President and both Secretaries of the Geological Society this year are graduates of St. John's College, Cambridge. For a long time this college has been prolific of geological students—indeed, we believe we might say that it has produced more geologists than all the other Cambridge Colleges together. This is probably owing in the main to the influence of Professor Bonney, when tutor of the college. Of the University honours in geology, all the Sedgwick Prizes except one, and all the Harkness Scholarships except two, have been won by Johnians.

ON February 14 the President of the College of Surgeons, Mr. Thomas Bryant, delivered the Hunterian Oration on the occasion of the centenary of the death of John Hunter. Mr. Bryant paid a high tribute to Hunter's efforts to establish surgery as a science, and referred to his wide knowledge of zoology and comparative anatomy. The Prince of Wales and the Duke of York honoured Mr. Bryant by attending.

WE learn from the *Athenæum* that a Natural History department is being arranged in the Imperial Museum at Constantinople.

Mr. Greene Smith's collection of North American birds and of Humming Birds has been given by his widow to the Museum of Comparative Zoology, Harvard College.

The plans of the new building for the Departments of Comparative Anatomy, Palæontology, and Anthropology in the Paris Museum of Natural History have been approved, and it is expected that the extension will be completed by the summer of next year.

We regret to record the sudden death, on February 14, of Sir Charles Wathen, through whose generosity the transfer of the Bristol Museum and Library to the city is being effected. On the proposition of Sir Charles, the Bristol Town Council unanimously decided, on the date mentioned, to adopt the Museums and Gymnasiums Act, but before the conclusion of the business the city's benefactor died suddenly from heart disease.

The annual report of the Curator of the Museum of Comparative Zoology at Harvard College for 1891-92 has just reached us. Professor Agassiz complains that "the time which our professors give to elementary teaching is entirely out of proportion to that allowed to them for higher instruction. Thus the facilities for original investigation which might be attained at the museum, and for what it was primarily intended, have been thrown away for many years, owing to the inability of the authorities to appoint men whose duties should lie in this direction." He complains that it is not the province of the museum to provide instructors; that belongs to the University; and he strongly protests against undergraduate instruction, which threatens to overcome the higher purposes of the institution. Professor Agassiz also has some strong remarks on the ignorance of those who, though unconnected with the staff, compile Government reports or "circulars of information." In this country the interference of the Government is confined to expenditure of money, on which matter Treasury clerks have power to overrule the decisions of the heads of the various scientific institutions. The description of the "Blake" and of the "Albatross" specimens goes on apace. The final portion of the report describes the progress of the Newport Marine Laboratory, of which some photographic views of the rooms are given, and it points out the advantages of certain specified extensions of this institution.

On the 18th of the month, the "Report of Proceedings with the Papers read at the Third Annual General Meeting, held in Manchester, July 5, 6, and 7, 1892," of the Museums Association was published. The volume (142 pp.) bears the misleading date of 1892, and is edited by Messrs. E. Howarth and H. M. Platnauer. The list of associates is small; thus we notice only three members of the staff of the British Museum (Natural History), but of these one is the Director, who, we are glad to learn, will preside this year—the Association meeting in London. The list of articles in the report is as follows:—The Organisation of a Botanical Museum, by F. E. Weiss; The Cultivation of Special Features in Museums, by Rev. H. H. Higgins; The Colouring of Museum Cases, by E. R. Waite; Museum Notes, by J. W. Carr; Dust in Museum Cases, by T. P. Teale; Arrangement of Rock Collections in Museums, by H. M. Platnauer; several articles relating to Art Collections, and, more important to the specialist, a "Catalogue of Types and Figured Specimens in the Manchester Museum," by H. Bolton. This last and exceedingly useful list is an outcome of the suggestions of a Committee formed at the British Association meeting of 1889, and adds another to the lists of types contained in the Bristol, Bath, Brighton, Cambridge, and other centres already published. But why did Mr. Bolton put in *Cyclus scotti*, H. Woodw., and *Myriolepis hibernica*, Traquair, as new species? In the first place, new species ought not to be described in a publication of this kind, and in the second place they have both appeared before this report was published (*i.e.*, distributed to subscribers), though the unwary are not given this information.

The Croonian Lecture for 1893 will be delivered before the Royal Society of London on March 16, by Professor Rudolph Virchow, of Berlin. The subject is "The Position of Pathology among the Biological Sciences."

Natural Science is represented in the programme of the Royal Dublin Society's afternooon lectures by Professor W. J. Sollas and Dr. V. Ball, the former of whom is to speak on "The New Geology," and the latter on "The Scilly Islands."

The study of ethnology and anthropology is being taken up in Ireland with some enthusiasm. An anthropometric laboratory has been, for several months, established in Trinity College, Dublin; it is under the supervision of Professor D. J. Cunningham and Mr. C. R. Browne. The latter observer joined Professor A. C. Haddon last autumn on an ethnological expedition to the Aran Islands, in Galway Bay, when numerous measurements of the islanders were obtained. Professor Haddon and Mr. Browne have lately contributed their results to the Royal Irish Academy. A course of popular evening lectures on Anthropology is in progress at the Royal College of Science, Dublin, by Professor Haddon, who has also been lecturing on the subject at Belfast.

The Natural History Society of Northumberland, Durham, and Newcastle-on-Tyne has organised a series of popular Saturday evening lectures, delivered in its Museum. Dr. Embleton, Canon Tristram, Professor G. S. Brady, and Professor M. C. Potter are among the lecturers, and the public are taking full advantage of the facilities afforded them.

The hundredth anniversary of the foundation of the Literary and Philosophical Society of Newcastle-on-Tyne was celebrated by a Conversazione on February 7. The President (Lord Armstrong) performed and explained some electrical experiments illustrating his recent researches; and the senior Secretary (Dr. R. Spence Watson) read a brief historical sketch of the Society. Next to the Literary and Philosophical Society of Manchester, which was founded in 1781, that of Newcastle-on-Tyne is the oldest in Britain. At the time of its foundation, there were, even in London, few learned bodies. The Royal Society was a hundred years old, and the Society of Antiquaries and the Society of Arts were at work, but the Linnean Society, founded in 1788, was the only association in England devoted to the investigation of any single branch of natural science. During the century of its existence the Newcastle Society had formed a great library, containing many almost unique works, and it is sad to have to relate that, on the morning after the Conversazione, nearly the whole of this library, except the Reference Department, was destroyed by fire.

At the anniversary meeting of the Geological Society of London, held on Friday, February 17, Mr. Hudleston was re-elected president, and Mr. J. J. H. Teall elected secretary, in the room of Dr. Hicks retiring. Owing to the feeble health of the President, the ordinary official annual dinner was not held this year. In view of our remarks in January on "Scientific Dinners," we are glad to observe that Dr. Henry Woodward, a vice-president of the Society, initiated a less costly entertainment, which, we understand, was quite informal, and was attended by no less than 90 Fellows and their friends. Dr. Woodward occupied the chair, and was supported by Sir A. Geikie, Sir H. H. Howorth, Professor Maskelyne, Dr. Hinde, Professor Rupert Jones, Professor Lapworth, and the secretaries of the Geological Society.

OBITUARY.

FRANCIS ORPEN MORRIS, M.A.

BORN MARCH 25, 1810. DIED FEBRUARY 16, 1893.

BRITISH Naturalists will learn with regret of the death of the Rev. F. O. Morris, which took place at his residence, the Rectory, Nunburnholme, Yorkshire, on February 16. Of those devoting themselves to the spread of an interest in Natural History pursuits among the unlearned, Mr. Morris had long been a conspicuous figure; and his success was gained both by the charm of his personal enthusiasm and by the numerous, well-illustrated popular writings which enabled him to reach a wider circle. His best known and most valuable works are his "Histories" of British Birds, their nests and eggs, and of British Butterflies and Moths, the publication of the first commencing in 1851. The new school of Biology inaugurated by Darwin and Wallace, unfortunately, never found favour with Mr. Morris, and his long series of miscellaneous writings antagonistic to this school began with a small volume on "The Difficulties of Darwinism," published in 1870.

CORRESPONDENCE.

A HYPOTHETICAL EXPLANATION OF THE RESULTS OF INOCULATION.

MR. BULMAN sets forth (NATURAL SCIENCE, vol. ii., pp. 100–109) the arguments for and against two theories—the "Exhaustion Theory" and the "Antidote Theory." Another explanation has been familiar to me for some time and to this he has not referred; I greatly regret that I am unable to say where I got it from, but I rather fancy some part of it was "evolved out of the depths" of my own "inner consciousness."

It is as follows:—

The attenuation of the virus by cultivation under conditions unfavourable to the microbe's very rapid multiplication amounts to an artificial selection, the too-rapidly-multiplying stocks being exterminated by overcrowding and the poisonous effects of their own products. After several cultures, each raised from the survivors of the preceding ones, an "attenuated virus" is obtained, *i.e.*, a pure culture of those stocks whose rate of multiplication is low.

The selection may, however, act in some other way—the net result being that the final culture consists of those stocks which, however they may behave on gelatine or boiled potatoes or in chicken-broth or the blood of living apes or guinea pigs, are

different from those with which we started: *by selection* a new stock has been produced, in some cases it is more virulent, in other cases less so.

The dependency of the protective influence of inoculation upon the rate of multiplication of the pathogenic microbe will be seen when we leave ectasines and anectasines out of account and consider only phagocytosis.

Whatever the nature of the stimulus may be, Cohnheim's demonstration that diapedesis of leucocytes is the dominant phenomenon of the resultant inflammation, together with their action as phagocytes in presence of microbes, may be taken for granted. At the seat of inflammation, then, there is a terrific battle between two armies, the microbes attacking, the phagocytes defending. In both armies there is a great mortality, and the ultimate result of the battle depends upon the rate at which each army is reinforced—*i.e.*, upon the rate of multiplication of microbes on one hand and on the rate of migration of fresh phagocytes to the scene of action on the other. So long as the microbes have the best of it the fever increases. So soon as the phagocytes become sufficiently numerous to cope with the microbes, destroying them more rapidly than they are produced, so soon does the fever begin to abate.

The hypothesis in question is that the stimulus of the poison not only increases the diapedesis of leucocytes (*i.e.*, transportation of the defending army of phagocytes to the scene of action), but also leads to an increased rate of production of leucocytes within the body, so increasing not only the size of the standing army provided for the defence of the body, but also increasing the rate at which new recruits are added to it.

If the microbe army introduced be only an "attenuated virus," then that army will not have time to increase to unmanageable dimensions before the phagocytes appear on the scene; the disease will, therefore, be easily checked, and the net result will be an increase in the strength of the army of phagocytes, whose duty is the protection of the animal against the attacks of armies of microbes.

I am not a pathologist, and only bring this to Mr. Bulman's notice that he may subject it to that same criticism which has apparently destroyed the two theories with which he dealt in his article in the current number of Natural Science.

C. Herbert Hurst.

Owens College, Manchester,
February 13, 1893.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., 29 *Bedford Street, Strand, London, W.C.*

All Advertisements *to be forwarded to the sole agents,* John Haddon & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ERRATA.

P. 86, line 39. For "pashs" read "parks."
P. 87, line 20. For "Oházal" read "Dhúyal."

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 14. Vol II. APRIL, 1893.

NOTES AND COMMENTS.

Prehistoric Man in Switzerland.

Those interested in the study of prehistoric man should read an important paper which has just appeared in the "*Nouvelles archives des Missions scientifiques et littéraires*" (vol. iii., pp. 1–25, pls. i.–iv.) In this paper M. Marcellin Boule gives a clear and readable account of the excavations undertaken by Dr. Nüesch in deposits under a rock-shelter at Schweizersbild, near Schaffhausen, in Switzerland. The explorations are particularly valuable, not only from the interest of the objects discovered, but on account of the care that has evidently been taken to note the exact conditions under which each object was found.

The highest deposit is of recent origin, and contains Neolithic and Palæolithic implements mixed pell-mell with coins, etc. Bed 2 is entirely of Neolithic age, and yields not only implements, but graves with human skeletons (nine have already been found.) It contains, also, bones of various animals, all still living in Switzerland with the exception of the reindeer, the bones of which, however, are thought by M. Boule to have been dug out of the lower strata when the Neolithic graves were excavated. Next comes a rubbly deposit, without trace of man, but containing a loamy seam full of bones of small rodents. Though this bed 3 is of great importance, separating as it does Neolithic from Palæolithic strata, we are not told what the rodents are, or whether they point to warm or cold conditions. The omission is unfortunate, for we should like to know whether any climatic change coincided with the change of races. In bed 4 the reindeer is the dominant form, and with it are found bone tools, har

poons, and needles, hearths and traces of cookery, and worked flints of the patterns commonly found in deposits of the reindeer period. A few sketches of horse and reindeer, incised on flat pieces of bone or limestone, have also been discovered. Bed 5 has as yet yielded no trace of man, but is extremely rich in remains of rodents, like those characteristic of the extinct German "steppe-fauna" of Professor Nehring, who has also identified the specimens from Schweizersbild. Directly beneath lies a mass of rolled pebbles, forming part of the widespread sheet of flood and moraine gravel usually referred to the latest glaciation of the Rhine valley.

These excavations show that part at least of the Palæolithic deposits are newer than the last glaciation of the district. We have, however, no account of the contents of bed 3, which may possibly show a less marked recurrence of arctic conditions, such as seems to have taken place in Britain after the Palæolithic period and before the incoming of Neolithic man. The oldest types of Palæolithic implements, and the large extinct mammals, such as elephant and rhinoceros, usually associated with them, have not been found at Schweizersbild, and it is still a moot point whether they should be looked for above or below the moraine gravels which mark the climax of the Glacial period. It is satisfactory to learn that half the area under the rock-shelter at Schweizersbild remains to be excavated, so that there is still a possibility of clearing up these doubtful points. We shall watch with great interest the results of Dr. Nüesch's excavations during the coming season, after which we hope to receive a fuller report.

Early Flowering of Plants.

Now that the season has begun, we would like to suggest the advisability of a more scientific method in the observation of dates of flowering of plants. One constantly comes across notes recording the exceptionally early appearance of certain flowers. It is forgotten, however, that the premature opening of the flowers may often mean death to the plant, or at any rate a serious waste of material. The production of seed is the final end to which all parts of the plant are modified. If by early flowering the plant is enabled to seed more freely, the date of flowering will tend to become earlier and earlier; but if the flowering is premature, and is not followed by the formation of seed, then the date will become later and later, as the too hasty individuals are killed off.

Observers should not pick these early flowers, they should mark the specimens and return later on to see whether they have, or have not, produced any seed. No one appears yet to have noted whether the buttercups and dead-nettles, which flower more or less all through the winter, produce seed from the winter flowers.

The Great Barrier Reef of Australia.

In our issue for November, we called attention to the work of Mr. Saville Kent on the Great Barrier Reef, and were enabled, through the courtesy of Messrs. W. H. Allen & Co., to give a specimen plate from his remarkably interesting book. We have now received from the publishers a set of twelve beautiful enlargements in permanent photography that have been prepared for the use of Museums and Natural History Societies. These pictures, which measure 15 ins. by 11 ins., show, in a striking manner, the appearance of a coral reef at low water, and, in many instances, even the generic relation of the coral masses can be determined. From a geological or zoological point of view, these photographs will be invaluable. They are published, ready mounted for framing, at half-a-guinea each, or four guineas for the twelve.

Dredging on Rocky Ground.

So much has been written about our knowledge of the deep-sea and its inhabitants, that it is scarcely realised how little we do know. We let down a dredge or trawl at haphazard into the dark, and scrape off certain of the creatures that are to be found on the surface of the muddy bottom; but of those that bore deeply we have no knowledge. On rocky or stony ground we have not even these small opportunities for learning what creatures live there, except the few that may be brought up by the hempen tangles; yet in all probability it is on the irregular rocky bottom, rather than on the monotonous mud flats, that life is the most varied.

If we want to obtain some measure of the extent of our ignorance, we must put ourselves in the place of the inhabitants of the deep-sea. Let us imagine some dwellers in upper air to be anxious to learn what sort of creatures are to be found in its lower strata, or crawl about on the earth's surface, beneath the mantle of cloud and fog. An exploring balloon is sent out, with instructions to dredge carefully. The first haul is made in the dark on rocky ground—say among the houses of London. What would be the result? The dredge would leap from roof to roof, missing the varied life of the streets below. It would bring up some slime out of the gutters, a tangled mass of telegraph wires, some weather-cocks and ventilators, broken chimney pots, a stray cat or two, and perhaps a casual burglar with his luminous apparatus.

The scientific men of upper air, asked to report on the haul, would remark on the wonderful advance that had been made in the knowledge of the deeper air, and of the customs of its inhabitants. They would reconstruct in imagination the world below, and would probably suggest that telegraph wires and chimney-pots are the snares and traps used by burglars for the capture of cats.

No doubt scientific men are very sensitive to any remarks on their ignorance; but we feel impelled to make these comparisons for their own sakes. It should clearly be recognised that the study of the habits of marine animals and of the interdependence of the various species in any fauna, needs something beyond museum work. Properly to understand the natural history of the deep-sea we must adopt a different method of procedure; we must examine the creatures alive, and under natural conditions. This has no doubt partially been done with a few of the species that will live in aquaria; but most marine animals have never been studied in the living state and cannot thrive in confinement.

Submarine Photography.

There seems, however, to be an unused method of research, which ought to yield satisfactory results, and should help us to understand what sort of life is led by the inhabitants of the deep-sea. Photography could, we believe, be used for submarine exploration without much difficulty, and we are surprised that so little has yet been done. Our ordinary photographic lenses would, of course, be useless for this purpose; but lenses to act through a denser medium could easily be made. In the second place, the pressure at considerable depths is so great that a camera of the usual pattern would not do; we must have the camera full of water, and with no air-spaces anywhere. This brings us to the last, and probably most serious, difficulty. How can the sensitive plates be preserved from injury if they are immersed in water? It could be done by laying another glass plate on the film, and photographing through glass, though this would tend somewhat to blur the image. A better way would be, if possible, to waterproof the sensitive film, and dissolve away the varnish before developing the plate.

By some such mode of procedure it is probable that we could obtain photographs as far down as light penetrates. We might even use the electric spark to illuminate the surroundings at great depths; for a hollow sealed sphere of thick glass will stand an enormous pressure. For the study of the manners and customs of the deep-sea fishes, a few sub-aqueous photographs would be invaluable. Coral-reefs also, though so carefully studied and well photographed down to low-water mark, are not understood. A series of photographs of the steep, submerged portion of the reef ought to throw a great deal of light on the vexed question of the origin of coral islands. We hope that some enterprising amateur photographer will take up this question, and begin by photographing in their native haunts the inhabitants of some of our lakes or sheltered sea-lochs.

Fruit-Growing at the Cape.

Fruit-Growing at the Cape is in an unsatisfactory way, according to Professor Macowan's account in the January number of the

Kew Bulletin. The methods of the fruit farmers are "antiquated and conservative," they lead an isolated life, and there is no effective interchange of ideas and information on the subject of their industry, nor do they care to learn what is being done in other countries, or might be done in their own. Moreover, the demand for cheap, coarse fruit among the coloured lower orders in Cape Town is a serious check on any desire for improving the output. "So that the grapes are dirt cheap it does not matter to them how dirty they are, nor are they disgusted at seeing the same baskets that carried the grapes into town piled up among the stable manure the cart takes back to the farm in the afternoon." So little is quality thought of that the producer can grow saleable crops from seedling trees fit only for stocks. "Upon the very few enlightened men who grow fruit on European principles, of the best sorts and in the best manner, the stolid indifference of the market and its pernicious ring of half-coloured middlemen reacts with cruel force. They cannot, somehow, reach the educated purchaser who wants fine and clean fruit, at its best maturity, fit for dessert." This is a sad state of things, and, moreover, the Professor tells us, it is the same with fish. "We all have to be content with what suits the Malays, dispensed Malay fashion." What a chance for someone, with a little capital and patience, to grow and retail his own fruit, and break this vicious circle of middlemen. According to Mr. William Tuck's report from Grahamstown, the growers are quite as casual in the Eastern Province.

The seasons on the two sides of the colony are differentiated, much as in India, by the rainfall occurring conversely, the west having its maximum in winter (June to August), while the east has generally two maxima in the warmer months, November or spring rains, and February or autumn rains. These peculiarities are important in fruit-growing, wine, grape, and raisin production being limited to the Western Province, where alone the summer is sufficiently hot and dry for the proper ripening of the fruit.

Atropin as a Plant Manure.

In a recent number of the *Revue Générale de Botanique*, M. Henry de Varigny describes some experiments on the value of the alkaloid atropin as a plant manure. Goppert (1834) was apparently the first to test the action of alkaloids in general, and atropin in particular, in this connection. He found the result to be the same whether he watered the earth, in which wheat, peas, oats, or cress were sown, with pure water or an infusion of belladonna. The infusion neither accelerated nor retarded germination.

P. B. Reveil (1865) came to a different conclusion. He used a solution containing a known quantity of the soluble sulphate of atropin, and found that it favoured the germination of barley sown

in calcined and washed sand. Moreover, for some plants, he does not say which, it is a "veritable manure"; when watered with a solution they were finer and more vigorous, and flowered better and more quickly than under normal conditions.

In 1887, however, M. A. Marcacci, in his "L'azione degli Alcaloidi nel regno vegetale et animale," asserts that atropin has an injurious effect on germination.

M. de Varigny has made an extensive series of experiments, growing his plants either on washed and heated sand or cotton-wool, and using solutions of the sulphate of atropin of various strengths, from $\frac{1}{15}$ to 1 per cent. Seeds of a considerable number of species were tried, wheat, barley, oats, and other grasses, cress, lettuce, radish, turnip, buckwheat, and others. In every case the experiments were checked by growing the seeds under conditions in every way similar, but without the alkaloid. As a result, he found that almost invariably there was certainly no advantage in the presence of atropin. For instance, several experiments were made with Indian cress, and in almost all the weight of the crop in the control experiment exceeded that in the others, and the greater the proportion of atropin the smaller the crop. This was equally well marked with wheat. Out of 75 seeds sown in each case, 64 germinated in the control experiment, 39 in the $\frac{1}{5}$ per cent. solution of the salt of atropin, and 41 in the $\frac{1}{15}$. In some cases, such as the beet, cabbage, lentil, buckwheat, and pea, the alkaloid had apparently no effect, germination and growth being almost the same in the two series of culture, while in the carrot and lettuce it certainly seemed favourable to growth, but M. Varigny thinks the experiments with these species must be repeated more frequently before arriving at any definite conclusion.

On the other hand, it is quite clear that there are a number of species whose germination and growth is retarded, or even stopped by the drug.

Jade in Upper Burmah.

Dr. Noetling, of the Indian Geological Survey, has issued an interesting report on jade in Upper Burmah. Speaking of the geology of jade, he says that two facts are now established—one, that jade is found in association with and enclosed in an eruptive rock closely resembling serpentine; the other, that this serpentine pierces strata of perhaps lower, but more probably of upper Miocene date. He observes that it is now proved that the jade found in Burmah belongs to a group of eruptive rocks of late Tertiary age, and as it is intimately connected with serpentine, it is probable that it will be found at other places where the latter occurs, when once the outer shell of the serpentine has been pierced. Formerly it was extracted only in the Uru Valley, where it was found in boulders in the alluvial deposits

of the river. Sometimes it was embedded in laterite, and was then particularly appreciated on account of the peculiar red crust which enveloped a core of jade. The boulders were found by digging holes along the banks of the stream, or by diving to the bottom; but recently an enterprising Chinese has imported a diving-bell for the purpose. At the quarries at Tawmaw the mode of extraction is primitive and destructive. The surface of the rock is heated by large fires, and the cold at night is sufficient to crack it without pouring on water. By using crowbars and wedges in the cracks, large blocks of jade are obtained, which are broken with mallets to make them of a size suitable for transport. This rude treatment naturally damages the stone, and therefore the alluvial jade is greatly preferred. Jade, says Dr. Noetling, is a curious example of articles highly prized by certain people, and regarded with complete indifference by others. The Burmese and the Chinese, especially the latter, value a good piece of jade as much as, if not more than, so much gold. Thus, they will pay for a piece large enough for a signet-ring 400 to 500 rupees, while the same piece sold in Europe will fetch little or nothing.

Anæsthetics and Plant Transpiration.

In the February number of the *Botanical Gazette* Albert Schneider gives an account of experiments on the influence of anæsthetics on plant transpiration. Jumelle concluded that sulphuric ether affected this function differently in the light and in the dark; in the former it was increased, in the latter retarded. The increase in the light was supposed to be due to the action of the ether on the chlorophyll bodies, while the retardation in the dark was not explained. Verschaffelt, on the contrary, maintains that ether increases transpiration both in the light and dark. Schneider claims that as these two experimenters used only parts of plants, their conclusions are of little practical value, the natural absorbent organ, the root, being absent. He himself uses small but entire plants of the potato, fuchsia, or geranium. Moreover, they may possibly have confounded *evaporation* with *transpiration*. The former—the mere passive loss of water—is much more rapid in the case of dead than living tissue, while the latter, the active giving up of water vapour, can only occur in living tissue, and is dependent upon protoplasmic activity. Prolonged contact with ether may kill the leaves of the plant under experiment and the result be confused by substitution of evaporation for transpiration.

Experiments on protoplasmic movements in the hair cells of *Primula sinensis* and other plants showed that ether vapour reduces their activity temporarily when exposed for a short time, and permanently if for a longer.

Experiments with whole plants of the potato seemed to show conclusively that exposure to the vapour of ether, amyl nitrite, or

chloroform materially diminishes the transpiration; fifteen minutes with amyl nitrite sufficed to kill the greater part of a plant, while it could withstand a much longer exposure to the other vapours. Ether and amyl nitrite reduced the transpiration both in light and dark, and the different colours of the spectrum had no effect on the influence of ether vapour. Red, yellow, and green lights were obtained by use of bell-jars, the hollow walls of which were filled with fuchsine, "diamond dye" yellow, or a mixture of diamond dye yellow and blue respectively, the conditions of temperature and moisture being constant. The average results of a series of experiments under otherwise normal conditions, showed a gradual slowing of the rate of transpiration from diffused white light, through yellow, red, and green, to darkness.

A set of experiments with the potato and a fuchsia showed that moisture did not affect the influence of ether and amyl nitrite; moreover, at no time, though the atmosphere was practically saturated, did the transpiration approach zero. Kohl, on the contrary, claims that in the saturated atmosphere it is zero.

Finally, to show the cause of Jumelle's apparently erroneous conclusions, Schneider made a series of experiments with leaflets of the potato. He found that, when exposed to ether for three hours or less, either in light or darkness, transpiration, estimated by loss of weight, was retarded, but with an exposure of $3\frac{1}{2}$ or 6 hours the loss of weight increased in both cases. Owing to the long exposures, the ether had stopped the protoplasmic activity, and hence increased evaporation in both cases, but most in the dark, because in the dark the leaflets were killed earlier.

The Landslip at Sandgate.

Sandgate, on the Kentish coast, was a good deal damaged by landslips on March 4 and 5, though the accounts of the mischief done have been considerably exaggerated. We are informed by Mr. Topley, who is intimately acquainted with the geology of the district, that the whole town is built on an old landslip, and that the western part of this disturbed mass has slipped again, owing principally to the exceptionally heavy rainfall experienced during the past winter. Mr. Topley will give an account of the landslip, and will exhibit photographs, at the meeting of the Geologists' Association, to be held at University College, on April 7.

Target Practice and Fisheries.

From the Report of the Target Practice (Seawards) Committee, just issued, we observe that some interesting evidence was given by Mr. Calderwood, of the Plymouth Marine Biological Station, as to the position of favourite fishing grounds on the South Coast. The Committee say: "The evidence of Mr. Calderwood . . . . satis-

fied us that it is of great importance that the military and naval authorities should be furnished with accurate information respecting the position of the favourite fishing grounds in the localities where target practice is carried on, and of the times of tide and seasons when fishermen in the neighbourhood are ordinarily engaged upon particular grounds, and of the character of the pursuits followed. And of the gear used by fishermen at different times of the year in fishing for various kinds of fish." Mr. Calderwood has furnished to the Committee, who have reproduced it in their Report, a map showing the localities for special fish off Plymouth during the month of April. Of course, these localities vary from week to week, and it would be desirable, and no doubt important, if a large series of such sketch-maps could be published. The evidence offered as to the effect of the electric light in attracting or frightening fish is conflicting, and the same may be said of the noise of the firing of heavy guns. We note that the index shows a great improvement on the general run of indexes to Blue Books, it is really a useful *précis* of the whole Report.

Tertiary Mollusca of Florida.

We have received from Mr. W. H. Dall the second part of his "Tertiary Mollusks of Florida" (*Trans. of the Wagner Free Institute of Science*, vol. iii., pp. 201–474). This part completes the account of the Gasteropoda, and places in our hands, at last, better materials for the study of former zoological provinces. No group of Tertiary fossils has been so well examined in many different parts of the world as the Gasteropods, and by a careful comparison of the different monographs that have lately appeared, we ought to be able to obtain information as to the former continuity or isolation of the different areas. An account of the Tertiary marine gasteropods of the Pacific slope of America is still wanted. Until they have been examined, it will be difficult to speak with confidence as to the dates of submergence of the isthmus of Panama, and we also cannot deal with the possible diversion of the Gulf Stream at different epochs, and its effect on the climate of Western Europe.

Tertiary Mollusca of Piedmont.

We are glad also to observe that the splendid monograph on the Tertiary Mollusca of Piedmont, left unfinished on the death of Professor Bellardi, is steadily progressing under the hands of Professor Sacco, of Turin. Another part has lately appeared, and we may expect before long to see the completion of the gasteropods. The Tertiary molluscan fauna of Italy is so prolific, that the monographing of those found even in a single region is a herculanean task. We hope that Professor Sacco will receive sufficient encouragement and

assistance to enable him to complete the work. When will the Palæontographical Society complete the monograph on our own Eocene Mollusca, left unfinished on the death of Messrs. Edwards and Wood?

Sea Pellets.

In the February number of the *Revue Générale de Botanique*, W. Russell has an interesting note on these peculiar felt-like balls thrown up by the sea, or sometimes found on the shores of inland lakes. They occur in great abundance round the Mediterranean and seem to have puzzled the old naturalists. The Greeks knew of them, for Galen and Aristotle recommend the use of their ashes as a remedy for scrofula. Later, Constant and Cornarius placed them in the genus *Alcyon*, a group which, however, included sponges and corals as well as sea-weeds. Cesalpinius, in his "De Plantis" (1583) describes them as formed of the down of the sea, but Imperato, nearly a century later, refused to rank them as plants, more nearly recognising their true nature as "balls of chaff and hair made by the rolling of the sea," and showing that they must not be confused with the hair-balls found in the stomach of goats and other ruminants.

At the end of the last century, Draparnaud found the pellets collected on the Mediterranean shores to be composed of fibres felted round a fragment of that curious marine flowering plant the sea-wrack (*Zostera*), and, later on, Bory de St. Vincent described similar ones from the Straits of Dover.

In 1857, Germain de Saint Pierre remarks on the spherical pellets of brown felted fibres found on the coast of Provence, and used by the hunters of the district as a wad for their guns, and finds that they consist "of fibres persisting on the stems of *Posidonia Caulini* after the destruction of the leaves, of which they represent the nerves."

Weddel, in 1867, explained their formation thus: The old root-stocks of the plant are torn into shreds and broken by the continual shock of the waves, while larger fragments rolled up and down the carpet of fibres, especially where the shore forms a gentle slope, catch them up, and become the nucleus of often very large balls.

Frequently, however, the core is absent, as in many of the cases quoted by Russell from Italy, where they were usually spherical or ovoid, solidly felted and remarkably homogeneous, but sometimes fusiform with a rigid axis, easily recognisable as a fragment of the rhizome. The author goes on to describe some pellets collected in the Island of St. Marguerite, which he has found to consist of scales of pine cones in all degrees of alteration, some intact, others reduced to a few fibres, and numbers of filaments generally rather thick, and only 5 or 6 centimetres long at the most, but mingled with others long and threadlike but less numerous. In the short filaments the bordered pits and resin canals characteristic of the pines were easily seen under

the microscope, but the threads were of a quite different structure, consisting of long cells, almost all of the same form, and, according to the author, evidently belonging to a monocotyledon presumably *Posidonia*, numerous rootstocks of which occurred on the beach mixed with the pine cones. These long filaments had played the part of a thread uniting the smaller *débris*, which otherwise would probably have remained separate.

Other cases are mentioned in which the *Posidonia* fibres act in this way. Bits of sponge may form a nucleus round which they group themselves, while seaweeds may get involved. An example exists in M. Bornet's herbarium, a green alga in the shape of a ball, in which the *Posidonia* filaments are caught like pins. The latter example is comparable with the instances cited by Masters, and found in certain small English lakes, of Confervæ interlacing and forming a ball with the leaves of the larch.

These recall the so-called aegagropilous algæ, which are merely species of the green alga *Cladophora*, detached at an early stage from their support, and rolled by currents. As Russell suggests, all submerged bodies, under certain circumstances, might form similar pellets. The pine needle balls from the lakes of the Engadine are well-known, and M. Jaccart, of the Lausanne Museum, states that in a little creek in the Lake of Geneva, where the water is unceasingly disturbed by currents, fine felted pellets may be seen formed by shavings from a saw-mill.

Evolution of Premolar Teeth.

That indefatigable worker, Professor W. B. Scott, of Princeton, has contributed to the last issue of the *Proc. Acad. Nat. Sci. Philadelphia* (1892, pp. 405–444), a very interesting paper on the evolution of the premolar teeth of mammals, especially those of the Ungulates and their allies. It is clearly shown that premolars start as simple cones, and receive their first accression by the addition of a smaller cone on the inner side. It is thus evident that the protocone in these teeth is external; whereas, according to the general interpretation, in the true molars it is an internal element. Professor Scott is thus led to conclude that in Ungulates like the horse, when the upper premolars exactly resemble the molars, the various cusps of these two series of teeth are not homologous with one another. He, therefore, proposes a new series of terms for the premolar cusps.

The idea that such precisely similar teeth have a totally different origin for their cusps is sufficiently startling to make us anxious to ascertain whether the inductions on which the theory rests are well founded. Now, at the conclusion of his paper, Professor Scott mentions that Herr Röse has recently come to the conclusion that in quadritubercular upper molars the cusps have been wrongly identified, and that the protocone is, after all, external. If this be so, adds Pro-

fessor Scott, the premolar and molar cusps are really homologous, and the long series of new terms proposed for the cusps of the former quite superfluous. The obvious comment on this is *festinâ lente*.

The Expedition to Lake Rudolph.

Advices have been received from some members of the Villiers Expedition to Lake Rudolph. This expedition, it will be remembered, left England last October for the purpose of exploring the country around the Lake, going through southern Somali-land, and returning through the northern part of that "horn" of Africa. At the same time, comes a despatch from Sir Gerald Portal to the Earl of Rosebery, containing the astonishing information that "This afternoon we reached Sambaru, 35 miles from the coast, and here have been overtaken by Lieut. Villiers, *who informs me that he has been authorised to attach himself to this expedition as one of my staff*, defraying, however, his own expenses throughout." We should like to know *who* authorised Lieut. Villiers to leave those whom he had taken out from England under promise of leadership, and who will be responsible should harm come to any of his followers?

Phosphates from India.

Sir J. B. Lawes has recently received, through the India Office, a consignment of Phosphates from Madras, with a view of their commercial value being ascertained. The specimens, of which we have been favoured with samples, are well-formed nodules, with a nearly smooth buffish coat, and internally appear very pure. Unfortunately, they show no traces of fossils; but they come from Utatur, where there are both Cretaceous and Eocene Beds.

Our Monthly Selection.

One does not expect accurate science in an ordinary newspaper, but yet it is not unreasonable to look for a general knowledge of the natural productions of our colonies in a paper supposed to be devoted to their interests. The following is taken from *The Colonies and India* of March 11, 1893, but we can assure would-be emigrants to New Zealand that they need not be afraid of meeting one of these birds, resembling an ostrich, but very much larger. Unfortunately for science, the Dinornis has been extinct for many years:—

"Dr. Reichenow read a paper at the last meeting of the Ornithological Society of Berlin, in which he gave some particulars of the finding of remarkable remains of gigantic birds in Argentina, double the height of the ostrich, which represent our living cassowaries and ostriches. In modern times many kinds of birds are becoming extremely rare, especially the dinornis races of New Zealand, which

are beginning to die out. Dr. Reichenow presented a specimen of one of these birds, a very rare and costly *Apteryx hasselti*, which inhabits the northern island of New Zealand, and while resembling an ostrich, is very much larger."

Dr. Nikitin has issued his seventh Record of the Geological Literature of Russia (for 1891). This record contains 452 items, the titles and a *précis* of the contents being given in Russian and French. Dr. Nikitin must be congratulated on the issue of this most useful work, and especially on the French translation, but for which, to the majority of foreigners, the rich geological literature of Russia would be unavailable. The most gratifying features, perhaps, in the book are the numerous and voluminous reports issued by the Governments on the nature of the soil and its agricultural value, and other matters of applied geology. This is the true aim and end of geology as far as the public is concerned, and we would like to see some works of this nature issue from our own Governmental office.

We are glad to observe, in the "Report of the Committee on the Present Condition of the Ordnance Survey," noticed under "Some New Books," that the Commissioners state (p. xxxii.) "that the absence of a path on the Ordnance Survey Map is no proof that there is not a right of way." This is a considerable improvement on the legend which appears on some of the recent one-inch maps, which reads thus: "The representation on this map of a road, track, or footpath is no evidence of the existence of a right of way," and we hope the legend will promptly be altered in all cases to the revised version.

Mr. John H. Cooke has an interesting paper in the February number of the *Mediterranean Naturalist* on "Some evidences of the occupation of the Maltese Islands by Prehistoric Man." He describes the evidences obtained by previous observers, and then relates his own results from the exploration of the Har Dalam Cave in 1892. Mr. Cooke found a stone implement and a human metacarpal associated with *Ursus arctos*, *Cervus*, *Elephas* and *Hippopotamus*, together with fragments of pottery of prephœnician page. The main results have been contributed to the Royal Society, and to the *Geological Magazine* (December, 1892), but the collating together of all the observations makes an interesting and useful article.

The leech *Hirudo brevis*, described by Grube from Chili in 1871, has been lately examined by Blanchard, who has given his opinion to the Academy of Sciences, Paris (*Compte Rendu*, 27th February, 1893), that it should form the type of a new genus, which he calls *Mesobdella*. After describing in detail the structure of the animal,

Blanchard goes on to say, by its peculiar characters, *Mesobdella brevis* (Grube) connects in a remarkable manner the Glossiphonidea with the Hirudinidea. Among the last, it approaches more nearly the Hemadipsinæ, as much by its mode of life as by the disposition of its eyes; but it is clearly distinguishable from them, as from all the other Hirudinidea by the great compression of the somites. The existence of this intermediate form shows that these two families are derived from a common stock, from which the Glossiphonidea have apparently deviated less than the Hirudinidea.

MM. Chevreux and De Guerne have some interesting observations on Crustacea and Cirrhipedes commensal on the turtles of the Mediterranean in the *Comptes Rendu* of the Academy of Sciences, Paris, 27 Feb., 1893. The observations were made during some excursions on the "Hirondelle," the "Actif," and the "Melita," in 1892. To a specimen of *Thalassochelys caretta* were attached *Lepas hilli*, some young *Conchoderma virgatum*, and a *Platylepas bissexlobata*, as well as the following crustacea: 16 *Hyale grimaldii*, 1 *Platophium chelonophilum*, 1 *Caprella acutifrons*, 4 *Tanais cavolinii*, and 3 *Nautilograpsus minutus*. On another specimen of the same Chelonian, which was abundantly garnished with the Alga, *Polysiphonia sertularioides*, no less than 259 Hyalæ, and several hundreds of Caprettæ, with other forms, were found.

Messrs. Warne & Co. announce the forthcoming issue of a new serial illustrated Natural History, edited by Mr. Lydekker The publishers have purchased electros of the greater number of the beautiful engravings in the third edition of Brehm's "Thierleben," and these, with the addition of coloured plates, ought to make the work highly acceptable to the public, altogether apart from the text. A large portion of the work will be written by the editor himself, but the assistance of specialists in certain groups has been secured.

Grevillea for March shows a steady improvement on its predecessors, especially in point of the illustrations. Mr. Batters makes a new genus, called Giffordia, in memory of the late Miss Gifford. It is made out of *Ectocarpus*. That old convenient genus is slowly breaking up, and in time will occupy a place like that of *Conferva*—the mother of genera. The reason, in the present case is the valid one of the discovery, by Dr. E. Bornet, of differences in important particulars between the male and the female cells, while typical *Ectocarpus* is isogamous. A great deal of work tending towards the unsettling of the present grouping of genera in Phacophyceæ has been recently done. There are the remarkable observations of Dr. Bornet on *Nemoderma Tingitana* (Algues de Schonsbœ in Mem. Soc. Nat. Sc. de Cherbourg, 1892), and those of Miss Mitchell and Miss Whitting on

Splachnidium rugosum (Phycological Memoirs, part i.), besides many minor observations of great value when taken into relation with others. Mr. Buffham, in his excellent "Algological Notes" in this current number of *Grevillea* describes plurilocular sporangia of *Chorda Filum* (of interest in the connection just alluded to) and a new species of *Giffordia*. A most valuable observation is that of the conjugation of zoogametes in *Cladophora lanosa*. Mr. Buffham deserves very hearty applause for these admirable notes—the more so since they are put forward very modestly in the shape of notes.

A TRANSLATION of Von Kennel's paper on "The Affinities and Origin of the Tardigrada," will be found in the March number of the *Annals Mag. Nat. Hist.* Kennel agrees with Plato in considering that the Tardigrada can be brought into relationship with the tracheata Arthropods, but he does not agree with him in considering that they lie nearest to the root of the Tracheate stem, or that they show most clearly the transition from the Annelids to the air-breathing Arthropoda. He goes on to point out the many resemblances to be found with the dipterous larvæ, and says that though it is by no means his attention to put forward the dipterous larvæ as actual ancestors of the Tardigrades, no single form so simply and so readily enables the student to interpret the Tardigrade body.

MR. FOX-STRANGWAY'S Memoirs on "The Jurassic Rocks of Britain" (*Mem. Geol. Survey*), dealing with the Yorkshire areas, has just been issued, and we hope to give a notice of it as soon as the second part appears. In the meantime, it may be asked why the date 1892 is put upon the title-page? It is true that the preface is dated 28th April, 1892, but the tell-tale printer's date, December, 1892, occurs on the same sheet. Why this eight months' delay.

IN our January number, we referred to the visit of Sir H. Maxwell and Mr. Hasting to Thessaly, to investigate the value of Loeffler's method of destruction of field-voles by mouse-typhus. Sir H. Maxwell has an article on the subject in *Blackwood's Magazine* for March, in which he points out that the remedy is not effectual, on account of the difficulty of spreading the soaked bread, and, moreover, it is too costly to become general. Five francs' worth of the liquid suffices only for two acres, and to clear a farm of 6,000 acres would cost no less than £600 for typhus-broth alone, not counting bread and cost of labour.

IN his presidential address to the Geological Society of America (*Bulletin Geol. Soc. America*, vol. iv., pp. 179–190, February 27, 1893), Mr. G. K. Gilbert deals with a subject which has lately been discussed in NATURAL SCIENCE. He speaks of the permanence of

continents and ocean basins, and is strongly inclined towads the view that in the main they are permanent.

In the *Geographical Journal* for March, Dr. Hugh Robert Mill has a short paper on the Permanence of Ocean Basins.

In the *Journal of Botany* for March, Miss Barton makes progress with her "Provisional List of Marine Algæ of the Cape of Good Hope," and there is a paper by E. D. Marquand on the "Mosses of Guernsey."

There is a portrait and an interesting and full account of the career of Frederick Courteney Selous, the Nimrod of South Africa, in the March number of the *Review of Reviews*. The thirteen pages devoted to Mr. Selous bristle with adventures, and form altogether an instructive and readable record of the dangers and excitements of hunting big game. Mr. Selous is more than a hunter, for he has many times enriched the national collection with rare or new beasts; some of his most stirring experiences are illustrated in the article.

We learn from a telegram received from Berlin on the 15th inst. that Dr. Stuhlman, one of Emin Pasha's late companions, has arrived at that place. He has brought with him two female dwarfs from the Upper Ituri district of Central Africa, who will be examined scientifically by Professor Virchow. The telegram, however, does not contain any news of Emin.

Miss Agnes Crane informs us that the fifteen-spined sticklebacks (*Gasterosteus spinachia*) have just commenced their annual nest-building in the Brighton Aquarium.

Messrs. L. Reeve & Co. have in preparation a new work on the British Aculeate Hymenoptera from the pen of Mr. Edward Saunders, uniform with the same author's work on the Hemiptera Heteroptera just completed, and noticed in our last number.

On March 16, at a meeting of the Royal Society, Professor Rudolf Virchow delivered the Croonian Lecture, taking for his subject "The Position of Pathology among the Biological Sciences."

I.

The Mammals of Kilima-njaro.

GREAT interest attaches itself to Kilima-njaro, the mountain-mass in which equatorial Africa attains its highest development above the sea-level. At one time it was supposed, not unreasonably, that the investigation of its flora and fauna would materially assist in the solution of the problem of the former extension of northern forms of life into southern latitudes. Such has not proved to be the case, so far as our explorations have at present gone. But the study of the organic forms of Kilima-njaro, and of the high district that surrounds it, has, nevertheless, resulted in the discovery of many unexpected facts in distribution; and the recent publication of Mr. True's account of the mammals collected by a well-known American explorer—Dr. Abbott—in that region (1), invites us to offer a few remarks on that particular branch of its fauna.

The snows of Kilima-njaro were first observed, as is well known, by the German missionary, Rebmann, in 1848. Von der Decken, New, Fischer, and Joseph Thomson were the next four explorers who saw the mountain, and ascended its slopes to a greater or less extent; but, except some bundles of dried plants, and a few insects and bird-skins[1] (obtained by Dr. Fischer), little if any information as to its natural history was derived from these expeditions. In 1883 our ignorance of the fauna and flora of this specially interesting district was brought before the Royal Society and British Association by the writer, and a "Kilima-njaro Committee" was formed to endeavour to improve our knowledge of this subject. The result of the operations of this committee was the expedition of Mr. H. H. Johnston, in 1884. Mr. Johnston's instructions were to proceed direct to Kilima-njaro and pitch his camp there, high up, for six months, and to collect as much as possible in the vicinity of the snow-line. Unfortunately, Mr. Johnston, although he carried out the committee's directions as far as possible, was hampered by want of means. As he has explained to us in his most interesting and attractive narrative (2), being unaccompanied by European collectors, and failing to obtain the aid of native assistants, he was unable to accomplish all that could have been wished, in spite of his well-

[1] Among these were the first specimens of *Turacus hartlaubi*—a fine Touraco peculiar to Kilima-njaro.

known energy. But it is to Mr. Johnston that we owe our first acquaintance with the mammal fauna of Kilima-njaro, and, as will be seen by reference to Mr. Thomas's list, published in the "Proceedings" of the Zoological Society for 1885 (3), seventeen species of the mammals that inhabit this mountain thus first became known to us. The mammals obtained and observed by Mr. Johnston belonged mostly to well-known and widely-distributed species, such as usually fall into the hands of a first explorer; but Mr. Johnston made many good notes on the subject (4), and was the discoverer of the fine local form of the Guereza Monkey (*Colobus caudatus*), which is restricted to Kilima-njaro. Mr. Johnston remarks as follows on the monkey life of the district:—

"During eight months on the Congo, I only saw monkeys twice in a wild state, and that in one place only; and throughout my entire stay of sixteen months in West Africa, I can only remember six occasions on which I actually beheld these animals in a state of nature. On the other hand, I had scarcely left the East Coast to journey towards Kilima-njaro when monkeys showed themselves abundantly in the wilds.

"The first to attract my attention were the baboons, probably the species known as *Cynocephalus hamadryas*, *C. sphinx*, and *C. babouin*. They were generally found on the outskirts of native plantations, where they almost subsisted on the maize and other food-stuffs stolen from the gardens of their more highly-developed fellow primates. In the inhabited region of Kilima-njaro, generally known as the country of Caga, baboons were strangely abundant. They were generally in flocks of fourteen to twenty, of all ages, and both sexes. They were so little molested by the natives that they showed small fear of man, and, instead of running away, would often stop to look at me about twenty yards off, and the old males would show their teeth and grunt. I have frequently seen the natives driving them from the plantations, as they might a troop of naughty boys, and the baboons retreating with swollen cheek-pouches, often dragging after them a portion of the spoil. On one occasion, in a river-bed at the foot of Kilima-njaro my Indian servant, ordinarily a very plucky boy, met a troop of baboons, who, instead of fleeing up into the trees, came running towards him in a very menacing manner, and he was so frightened at their aspect that he took to his heels. The baboons followed, and, but that the boy forded the shallow stream, and put the water between him and his pursuers, he might have had an awkward contest. I killed a baboon once in Caga, one of a troop who were rifling a maize plantation, and its companions, instead of running away, surrounded the corpse and snarled at me. As I had fired off both barrels of my gun, and had no more ammunition, I went back to the settlement to fetch some of my followers, and upon the approach of several men the baboons ran off. We picked up the dead one and carried it back. It was a female, and apparently young and tender. Out of curiosity, I had its flesh cooked the next day and ate it, hoping in this lawful way to form some idea of the practice of cannibalism; I can only say that the succulence and quality of this creature's flesh were quite unexceptionable. I have noticed this with most of the species of Old World monkey I have as yet tasted. During my four months' stay in Mandara's country I ate the common *Cercopithecus* constantly, and found it made a very tooth-

some stew. The most remarkable monkey in all this region is probably the *Colobus*, which apparently offers a new variety or sub-species in the country round Kilima-njaro. remarkable for having an entirely white, heavily-plumed tail. The common species, with a black tail tipped with white, I have shot in the forested plains near the coast. The Colobus monkey is almost the only one that quite avoids the neighbourhood of man; the other genera frequent the vicinity of native plantations, and doubtless profit by the abundance of cultivated food.

Block lent by the [*Pres. Linn. Soc.*

Fig. 1.—Grant's Gazelle *(Gazella granti)*.

I never observed any *Galago* (a lemuroid animal) in this district, nor do the natives speak of one, although it is a genus well represented in other parts of Africa."

The sporting trip of Sir Robert Harvey and his friends to Eastern Africa, in 1887, and the consequent publication of Sir John Willoughby's "East Africa, and its Big Game" (5), resulted in another important contribution to our knowledge of the

mammals of the Kilima-njaro district, though no attempt was made to form a scientific collection. In the vicinity of what is well termed the "hunter's paradise" of Taveta, situated at an elevation of about 2,400 feet on the south-eastern slope of the great mountain, antelopes and other large "game-animals" were abundant. Speaking of one of his excursions from their headquarters, Sir John Willoughby says: "The grassy plain through which we marched was simply crawling with hundreds of *Gazella granti*, Wildbeest, Hartebeest, and Zebra." At that time, "any amount of game" was to be seen there, although these days are, we fear, already past and gone for ever. During their four months' stay in this earthly paradise, some 350 head of large "game" were obtained, amongst which were no less than 66 Rhinoceroses. The most abundant Antelopes in the district appear to have been the Coke's Hartebeest (*Bubalis cokei*), the Grant's Gazelle (*Gazella granti*), the Mpallah (*Æpyceros melampus*), and the Waterbuck (*Cobus ellipsiprymnus*).

The Hartebeest was "quite the most common Antelope on the plains, being found everywhere in immense herds." Grant's Gazelle, perhaps the finest species of this beautiful group, was also "common everywhere on the open plains," one male being accompanied by as many as from ten to fifteen females. The Mpallah is stated to be abundant in the bush as well as on the plains, and the Waterbuck to be "found everywhere near the rivers and marshes." Of other Antelopes met with by these fortunate sportsmen, we find the Eland (*Taurotragus oreas*) mentioned as "rather local." Both males and females in this district are more or less striped. The large Kudu (*Strepsiceros kudu*) was only seen on two or three occasions, and the Lesser Kudu (*S. imberbis*), though found in the bush near Taveta, is not common.

A very charming species, of which Sir Robert Harvey and his party were, I believe, the first to obtain perfect specimens, is Thomson's Gazelle, discovered by Mr. Joseph Thomson during his journey through Masailand in 1883, and discriminated by Dr. Günther from a pair of horns. This handsome little Gazelle was "found in large numbers in the plains to the south-west of the mountain, and occasionally mixed with herds of *Gazella granti*. Another graceful Antelope, "plentiful on the plains, and in thin thorny bush near Taveta," was the Oryx of the district. In his notes on the animals met with, Mr. Hunter refers to this Oryx as the Beisa (*Oryx beisa*); but, as has been recently shown by Mr. Thomas (6), the Oryx of Kilima-njaro is not the true Beisa, but an allied species, distinguished by its pencilled ears and different markings. Altogether, examples of sixteen species of Antelopes were obtained by Sir John Willoughby and his friends in the vicinity of Taveta, besides one or two others observed or heard of. At that epoch also the Giraffe and Zebra were very common in the plains near Taveta, and the Elephant was found in the forest of Kilima-njaro. Mr. Hunter's Appendix to "East Africa

Fig. 2.—Thomson's Gazelle (*Gazella thomsoni*).

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and its Big Game" also contains notes upon other mammals obtained and observed during this remarkable excursion.

A few months after Sir Robert Harvey and his friends had quitted their post at the foot of Kilima-njaro, a learned German geographer, Dr. Hans Meyer, accompanied by Herr von Eberstein, was engaged on an energetic attempt to climb the untrodden summit. On this occasion only the ice-cap was reached, at an elevation of about 18,000 feet, as it was found impossible to ascend further "without the aid of the usual Alpine climbing tackle." The results of this expedition were published in the work entitled "Zum Schneedom Kilimandscharo" (7), and illustrated by splendid photographs, which no one who wishes to form an idea of this remarkable mountain and its surroundings should omit to consult.

Nothing daunted by his comparative failure on the first occasion, Dr. Hans Meyer returned to the charge next year. In his second expedition he took as his companion a well-known Austrian geographer, Dr. Oscar Baumann, who had had a large experience of African travel on the western coast. Landing at Zanzibar in July, 1888, the travellers indulged in a preparatory excursion into Usambara—a high district opposite the island of Pemba, which forms the north-east corner of German East Africa; there, unfortunately, they fell into the hands of the revolted Arabs, and, after serious maltreatment, barely escaped with their lives back to Zanzibar and Europe.[2] Dr. Meyer's pluck and energy were, however, by no means exhausted by this sad event, and a third expedition was immediately planned and carried out. This time, Dr. Meyer determined to take with him an experienced Alpine climber, and was fortunate in securing the companionship of Herr Ludwig Purtscheller—a name well-known in Alpine circles. The event on this occasion fully rewarded Dr. Meyer for the trouble he had taken. All that had been left undone on the two first expeditions was fully accomplished on the third. The great crater of Kibo was discovered, the summit of the mountain at an elevation of 19,720 feet was attained, and named (of course) "Kaiser Wilhelm Peak," and an accurate survey of the whole district was carried out, besides which scientific collections in several branches of Natural History were formed. The results of this successful expedition were given to the world in 1891 in the splendid monograph entitled "Across East African Glaciers" (8), of the merits of which it is almost impossible to speak too highly.

Dr. Meyer does not seem to have paid much attention to the mammals of Kilima-njaro, although he made special collections of its butterflies and plants. He mentions, however, the occurrence of "The Eland" on the mountain at the extraordinary height of 15,000 feet. As, however, this animal was only recognised by its "footprints and droppings" in the boggy ground, I should rather imagine it might

[2] See Dr. Baumann's account of this perilous journey in his charming book "In Deutsch-Ostafrica wahrend der Aufstandes." Vienna: 1889.

Proc. U.S. Nat. Mus.] [vol. xv., pl. lxxvi.

Fig. 3.—Pencil-eared Oryx *(Oryx callotis)*.

have been the Koodoo, which Mr. Johnston also met with on Kilima-njaro (*op. cit.*, p. 354), and which Mr. Hunter believes that he recognised (at 9,000 feet) by "the crumbling core of an old horn."

Another passage in Dr. Meyer's work is of still greater interest as regards the mammals of Kilima-njaro. When climbing the ice-sheet of the Kibo crater from the west, at a height of 20,000 feet the travellers found the dead body of an antelope—one of the small species they had noticed on the pasture-lands below. This was, in all probability, *Cephalophus spadix*, discovered by Dr. Abbott, to whose labour we must now proceed to refer.

The American naturalist and explorer, Dr. W. L. Abbott, spent nearly eighteen months on Kilima-njaro and its vicinity in 1888 and 1889, and collected a splendid series of its mammals, which was presented to the U.S. National Museum at Washington. The specimens have been very carefully worked out by Mr. Frederic W. True, Curator of the Department of Mammals of that institution, and we must now devote our attention to his excellent memoir on this subject (1) lately published.

Mr. True tells us that Dr. Abbott's specimens are "prepared with much care, the skins being almost invariably accompanied by the skulls, and furnished with labels giving the locality and date of capture, sex, and other particulars." Adding to Dr. Abbott's series the names of the species recognised by former observers, Mr. True finds that the mammalian fauna of Kilima-njaro and the surrounding district includes about seventy species.

The Quadrumana of Kilima-njaro, according to Mr. True's list, are of four species—three *Cercopitheci* (this being one of the most abundant and most characteristic groups of Æthiopian Monkeys), and one *Colobus*—namely, *C. caudatus*—the localised form of *C. guereza* already alluded to. To these will have to be added a Baboon (*Cynocephalus*), of which Mr. Johnston saw numerous examples, and concerning which, I think, he can hardly have been mistaken, although we do not yet know the exact species.

Of the Lemurs, the only species yet recognised from the mountain is *Galago crassi-caudatus*, which Dr. Abbott found "common" in the forests of Taveta; but the natives state that there are three other kinds of these animals in the same district. One of them may be *Galago garnetti*, which the Zoological Society has received living from the Zanzibar coast.

The Carnivora are more numerous in Kilima-njaro. The Lion, Leopard, Serval, and Cheetah are all attributed to this district by Sir John Willoughby and his brother sportsmen, and, no doubt, correctly. The Leopard is stated to be common on the mountain up to six or seven thousand feet in altitude, and to be very arboreal in its habits. The Viverridæ are also well represented. Dr. Abbott obtained examples of seven species, among which is the Ratel (*Mellivora capensis*), said to be "rare upon the mountains," and a Genet

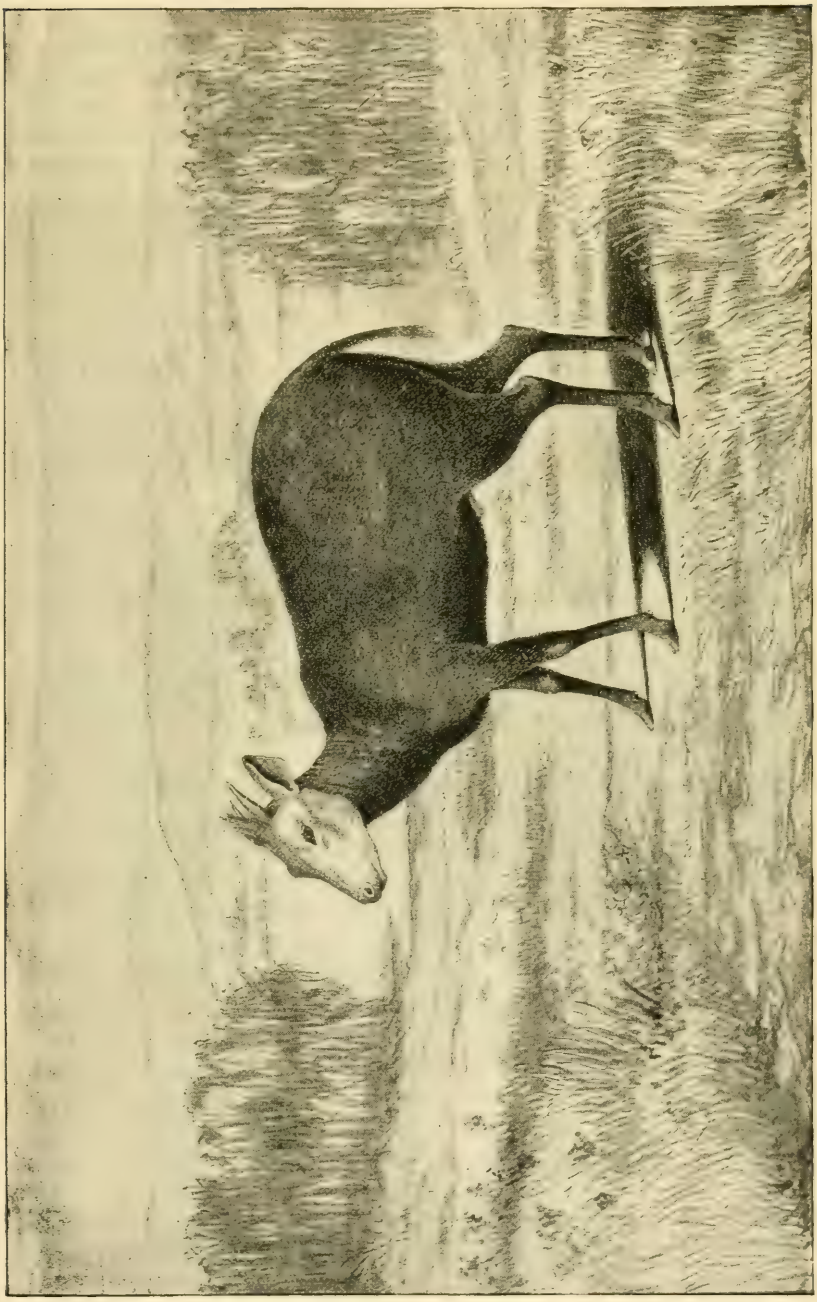

Fig. 4.—Abbott's Antelope (*Cephalophus spadix*).

[*Proc. U.S. Nat. Mus.*] [vol. xv., pl. lxviii.

(referred by Mr. True to *Genetta pardina*) which varies excessively in colouration. Mr. Hunter included both the Striped and Spotted Hyenas as met with on the plains surrounding the mountain, the former being "very common everywhere." The Canidæ of Kilima-njaro are not yet accurately made out. Mr. True refers Dr. Abbott's specimens of Jackals to *Canis mesomelas* of the Cape; Mr. Johnston and Mr. Hunter associate this animal with *Canis lateralis* of the West Coast. Mr. Hunter also speaks of a "Wild Dog" that "hunts in packs" on the plains, and of another "small, dark-coloured fox." The latter, however, may be *Otocyon megalotis*, of which Dr. Abbott obtained three specimens.

Two species of *Hyrax* are met with on Kilima-njaro, one belonging to the tree-loving and the other to the rock-frequenting section of this isolated group. Mr. True refers the former to a new species, *Dendrohyrax validus*, and the latter to *Procavia brucei* (Gray). He has not had the advantage of consulting Mr. Thomas's recently-published revision (9) of this very difficult family, so that the latter determination may be doubtful.

Of Rodents, Mr. True registers fourteen species in his list, and the number will, no doubt, be seriously increased when fresh explorers in Kilima-njaro turn their attention to these little animals. Among the Muridæ, a new species of *Dendromys* "extends the range of this genus from South Africa to East Africa."

The list of Kilima-njaran Bats is also very short up to the present time. Mr. True only mentions four species. It cannot be doubted that these creatures are much more numerous, but they are difficult to capture, and do not excite the enthusiasm of the ordinary collector. The Insectivora are not abundant anywhere. In Kilima-njaro only two species have been yet recognised. These are a Hedgehog (*Erinaceus albiventris*), and a Shrew (*Crocidura*).

We now come to the most highly-developed and interesting portion of the mammal-fauna—that is the Antelopes, abundant all over Africa in suitable localities, and especially so on the high plateau near Kilima-njaro. Mr. True includes the names of twenty-one species in his list, of thirteen of which examples were obtained by Mr. Abbott, while eight others had been recorded by Mr. Hunter, as already mentioned. The list must, however, be slightly reduced, as Hunter's Antelope (*Bubalis hunteri*) is only met with in Southern Somali-land on the north bank of the Tana, a long way from Kilima-njaro, and should be omitted altogether. Again, as Mr. True allows it is not at all likely that two allied species of *Madoqua* occur there, Mr. True's *Neotragus kirki* and *N. damarensis* are probably identical. Moreover, as shown by Dr. Günther (*P.Z.S.*, 1890, p. 604, x.), the Reed-buck of this part of East Africa is *Cervicapra bohor*, not *C. arundinum*, and the so-called *Cephalophus nigrifrons* is, no doubt, Mr. Thomas's recently described *C. harveyi* (xi.). This would make the Antelopes of Kilima-njaria at present known

about nineteen in number — namely, according to my nomenclature: —

1. *Bubalis cokei*.
2. *Connochætes taurinus*.
3. *Cephalophus grimmi*.
4. ——— *harveyi*.
5. ——— *spadix*.
6. *Madoqua kirki*.
7. *Neotragus moschatus*.
8. ——— *tragulus*.
9. *Cobus ellipsiprymnus*.
10. *Cervicapra bohor*.
11. *Æpyceros melampus*.
12. *Gazella granti*.
13. ——— *thomsoni*.
14. *Lithocranius walleri*.
15. *Oryx callotis*.
16. *Tragelaphus roualeyni*.
17. *Strepsiceros excelsus*.
18. ———— *imberbis*.
19. *Taurotragus oreas*.

It should be noted that the Brindled Gnu of Uganda and of this district seems to differ from the typical form of South Africa in having the throat-mane and jaw-tufts whitish or white, and has been made a sub-species by Mr. Thomas, as *Connochætes taurinus albojubatus* (xii.). There seems, however, to be no interruption in the range of this animal, which occurs, wherever the country is suitable, from the north of the Vaal up to Uganda. North of the Zambesi it has been recorded in Mozambique by Peters, in Nyassaland by Crawshay, and on the Kirgani River, opposite Zanzibar, by Speke. Mr. Hunter tells us it is found on the high plains N.E. of Kilima-njaro, but, unfortunately, calls it *Connochætes gnu*.

Besides the Antelopes, one of the forms of *Bos caffer* is abundant on the plains of Kilima-njaro, and, according to Dr. Abbott, as quoted by Mr. Hunter, ascends the mountain to an altitude of 10,000 feet. This completes the list of Bovidæ. The Giraffe was also (a short time ago) "very common round Taveta." Passing on to the Suidæ, both the Riverhog (*Potamochærus*) and Warthog (*Phacochærus*) are abundant in Kilima-njaria, and the Hippopotamus is found in Lake Jipe, and in "every large swamp." The Perissodactyle division of the Ungulates is represented by the Rhinoceros (*R. bicornis*) and one of the Zebras. I have always supposed this Zebra to be the northern form of Burchell's Zebra, with the legs striped outside (*Equus burchelli chapmanni*), but Herr Matschie has recently made a new name for it, *Equus boehmi*,[3] and he may possibly be justified in doing so. At the same time, there is great individual variation in the markings of Zebras, and it is very hazardous to base species on single skins and on sportsman's sketches.

The long list of Kilima-njaro Mammals is closed by the Elephant, which "lives in the thick damp forest, from 6,000 to 9,000 feet," in the dry season, descending to lower altitudes during the rains.

After this sketch of the Mammalian Life of Kilima-njaro, it will be evident that, so far as this part of its fauna is concerned, there are no traces of northern forms in Equatorial Africa, even at this excessive elevation above the sea-level. We might well have expected a Wild Goat to occur on the summit of Kilima-njaro, as it does on the mountains of Abyssinia.[4] Instead of a *Capra*, however, we find a

[3] *Sitz. Ges. nat. fr. Berlin*, 1892, p. 133. [4] *Capra walie*, Rüppell.

species of *Cephalophus*, a characteristic Æthiopian genus of Bovidæ. Nor, so far as we know at present, are there any traces of Marmots, Pikas, or Mountain-hares on Kilima-njaro. The places of these Boreal forms are taken by *Hyrax* and various species of Muridæ. In short, we find on Kilima-njaro merely more or less modified representatives of the inhabitants of the surrounding districts. So far as this piece of evidence goes, the wave of Boreal life, impelled by the Glacial Period, did not in Africa advance so far south as the Equator. What we *do* find in Kilima-njaria, however, is the most splendid series of the larger mammals to be met with in any spot on the earth's surface, such as may well justify Sir John Willoughby and his friends in terming their headquarters at Taveta the "Hunter's Paradise."

[Three of the illustrations prepared for this memoir have been kindly lent by the authorities of the U.S. Nat. Museum for the present article.]

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P. L. Sclater.

II.

Christian Konrad Sprengel.

FEW men have been more in advance of the age in which they lived, or have suffered so long an undeserved oblivion, as the subject of this brief sketch. No mention of him is made in the Biographical Dictionaries, nor in the histories of Botany prior to that of Sachs (1875). Had not his discoveries been re-enunciated and extended by Charles Darwin, his name might even now be wanting in the roll of famous botanists. Very little is known about his earlier life. Born in 1750, he became Rector of Spandau, near Berlin. There, under Heim, he began the study of Botany, and became so enthusiastic over it that he neglected his duties as Rector, and was ejected from his post. He migrated to Berlin, where he supported himself by giving lessons in languages and in botany, and took up a lodging on the Hausvoigtei-Platz in a back room at the top of the house. "Here," says one of his old pupils (from 1809 to 1813) in an enthusiastic account of him in "Flora" of 1819, "I always found him, in an old bedgown, with a nightcap and a long pipe, the room filled with clouds of tobacco smoke. He sat generally at the window, with a book or his herbarium. Shelves of books, his collection of plants, and a few old household goods completed the contents of the room."

He goes on: "In figure Sprengel was well-built, rather large than small, lean, and raw-boned. His face was full of expression, the colour fresh, the glance vivacious. He wore his hair, which was prematurely grey, uncut, hanging about his shoulders. His gait was firm and upright; he walked fast, and, in spite of his age, went for half-a-day without rest. He was simple and frugal . . drank only water . . was never married."

Sprengel's only contribution to botanical literature is his now famous work, "Das Entdeckte Geheimniss," which was published in 1793, just one hundred years ago. This book—of which more hereafter—was too far in advance of its time, and met with a chill reception, which did not encourage its author. Besides being despised as a visionary by the dry systematists of the Linnæan school who then held sway in botany, Sprengel seems to have been too fond of speaking the truth, regardless of consequences, and thus became very unpopular,

and more and more forced into the seclusion in which we have already seen him. He was beloved, however, by his pupils.

On Sundays he usually conducted botanical excursions to the country round Berlin; any one might join these trips on payment of two or three groschen. On these occasions Sprengel was not merely a botanist, but would give instruction in anything that turned up. "He explained equally well the inscription on a tombstone, the construction of a windmill, the course of the stars, or the structure of a plant. . . . Knowing the country well, he led us always to places where rare or remarkable plants were to be found. There were few places where he had not himself found something new, or noticed something special, and he gladly took the opportunity of leading us thither. Thus he showed us at one place the divided sexes of *Mentha aquatica*,[1] which he had noticed there first, and afterwards observed in other species of Mentha. . . . In the Zoological Garden the Scrophularia gave him opportunity to explain dichogamy (see below). He was firmly convinced of the fertilisation of most flowers by insects, and he could, on this theory, so clearly explain the structure of flowers that it was a pleasure to watch and to listen to him. . . . The commonest plant became new by what he had to say about it; a hair, a spot, gave him opportunity for questions, ideas, investigations. Much remained a mystery to him; he was most exercised over the structure of *Parnassia*. Here he was unable to catch nature in the act."

Towards the end of his life he abandoned botany and devoted himself entirely to languages; he took up English, and was full of its great advantages. "He often said that Linnæus did not understand Greek, and had brought many errors into the nomenclature. He also blamed Willdenow for introducing the long and incorrect name *Pelargonium*, which should have been *Pelargium*." He died in Berlin in 1816 at the age of 66.

The chief interest of Sprengel's life centres in his famous book "Das Entdeckte Geheimniss der Natur im Bau und in der Befruchtung der Blumen" (The Secret of Nature discovered, in the structure and the fertilisation of flowers). This was published in 1793 as a quarto volume of 222 pages, with 25 large copper plates, the latter being wonderfully accurate and well executed. In this volume Sprengel gives details of his observations on most of the wild flowers round Berlin, and many cultivated ones, all leading to the general conclusion that flowers are, mostly, adapted for fertilisation by insects. He shows how the insects are attracted, rewarded for coming by the honey, &c., and how almost every minutest structure in the flower, down to hairs and spots on the corolla, can be explained

[1] This plant is gynodioecious, *i.e.*, bears upon some plants large hermaphrodite flowers, and on other plants smaller female flowers with aborted stamens. The same phenomenon is seen in many other plants of the order Labiatæ.

with reference to insects. The best way, perhaps, of treating the subject will be to let Sprengel speak for himself. His book begins as follows :—

"In the summer of 1787 I was carefully studying the flower of the wood crane's bill (*Geranium sylvaticum*) and I found that the lowest part of its petals was provided, on the inner side and the edges, with fine soft hairs. Convinced that the wise Author of Nature had not created a single hair without a definite end, I reflected as to what purpose these hairs might serve, and it soon struck me that if one supposed that the five honey drops, which are secreted by the same number of glands, were designed for the nutriment of certain insects, one would find it, at the same time, not improbable that care should be taken to keep the honey from being spoiled by rain, and that to attain this end these hairs were created. Every honey-drop sits on its gland immediately under the hairs, which occur on the edges of the neighbouring petals. Since the flower stands upright, and is pretty large, raindrops must fall into it during showers. None of the drops, however, can reach and mix with a honey-drop, being prevented by the hairs, just as drops of sweat are prevented from running down into the eye by the eyebrows and eyelashes. An insect, on the other hand, will not be in any way hindered from reaching the honey. After this I investigated other flowers, and found that several of them had something in their structure which seemed to serve the same end. The longer I pursued this research, the more I perceived that those flowers which contain honey are so arranged that, whilst insects can easily reach it, it cannot be injured by rain. I concluded, therefore, that the honey of these flowers, at least primarily, is secreted for the benefit of insects, and, that they may obtain it pure and uninjured, is protected from rain.

"In the following summer I investigated the forget-me-not (*Myosotis palustris*), and I found that not only does this flower contain honey, but that the honey is fully protected from rain. At the same time, however, I was struck by the yellow ring which surrounds the mouth of the corolla tube, and contrasts so beautifully with the azure blue of the limb. Is this circumstance also, thought I, to be referred to insects ? Has Nature coloured this ring for the express purpose of showing insects the way to the honey receptacle ? I examined, with regard to this hypothesis, other flowers, and found that the majority supported it. For I saw that those flowers whose corolla, as commonly happens, is coloured differently in one place than in the rest, always have these spots, figures, lines, or dots of special colour at the place where the entrance to the honey is found. Now I reasoned from the part to the whole. If, I thought, the corolla is coloured in special places on account of insects, its general colour must also be on their account ; and if the special local colouring serves to direct to the honey an insect which has already alighted upon the flower, the general colour of the corolla must serve to attract

from afar, by showing the presence of honey, the insects that are swarming about in the air in search of food.

"Examining, in the summer of 1789, some species of *Iris*, I soon found that Linnæus had erred in regard both to the stigma and the nectary; that the honey is fully protected from rain; and, finally, that there is a specially coloured place, leading insects to the honey. But I found more than this, namely, that these flowers cannot be fertilised at all except by insects, and then only by insects of fairly large size. Although I did not, at the time, find this idea confirmed by experience (this occurred in the following summer, when I actually saw humble-bees creeping into the flower), still I was convinced of its truth by the general appearance. I endeavoured, therefore, to discover whether other flowers are also so constructed that they can only be fertilised by insect agency. My investigations convinced me more and more that many—perhaps all—flowers which contain honey are fertilised by insects which feed upon the honey, and that, consequently, this nourishment of the insects is, in regard to them, the final end, but, in regard to the flowers, is only a means, and, indeed, the sole means, to a definite end, viz., their fertilisation, and that the entire structure of such flowers can be explained, if, in their investigation, one keep always in view the following principles:—

"1. These flowers are fertilised by this or that species of insect, or by several.

"2. This also will occur, that the insects, as they visit the flowers for honey, and in consequence of this, either rest upon the flowers in some indefinite way, or in a definite manner either creep into the flowers or run round upon them, will, of necessity, be smeared with pollen from the anthers, either over the greater part of their hairy bodies, or only over a part of them, and will rub this pollen upon the stigma, which, for this end, is either covered with short and fine hairs, or with a damp, often sticky, secretion.

"In the spring of 1790 I perceived that *Orchis latifolia* and *O. Morio* have, in all respects, the structure of a honey flower, but do not contain honey. I thought at first that this observation, if it did not actually overthrow my former discoveries, would at least make them very doubtful. For, since these flowers have a honey guide (so have I termed the differently coloured spot on the corolla)—and yet this cannot show insects the road to the honey, as there is none[2]—it appeared to follow that the honey guides also, on those flowers which do contain honey, were not really for this purpose at all, and that the whole thing was a mere fancy. I must, therefore, confess that this discovery was in no way pleasant to me. But just this spurred me on to study these flowers more attentively, and to observe them in the field, and at last I discovered that these flowers are fertilised by

[2] Darwin showed that insects visiting Orchis drill holes into the tissue of the spur and suck the juice therein contained, so that the flower is not really an exception to Sprengel's views.

certain flies which, deceived by the appearance, imagine the spur to contain honey, and creeping in, tear the pollen masses from their chambers and bring them upon the sticky stigma. Flowers of this type, which have fully the appearance of honey flowers, but do not contain honey, I call 'sham honey-flowers' (Scheinsaftblumen). That there are more of them, I saw in the same year on the common birthwort (*Aristolochia Clematitis*). I found, namely, that this flower also, which contains no honey, is formed in every way like a honey flower, and, on this account, small flies of all kinds creep into it. In the following summer I saw clearly that this flower is a true wonder of nature, namely, that these flies are led by the appearance of the flower to creep into it, that they may fertilise it, and that they are held prisoners in it until they have done so, but that as soon as this has occurred they are released from detention.

"In the summer of the year above mentioned I discovered, in *Epilobium angustifolium*, something upon which I had never happened before, namely, that this hermaphrodite flower is fertilised by humble and hive bees, not, however, each flower by its own pollen, but the older flowers by means of pollen carried by insects from the younger.[3] This observation shed great light upon many of my earlier discoveries. I was especially pleased when I found a similar method of fertilisation in the wild love-in-a-mist (*Nigella arvensis*). In the summer of 1788 I had perceived the beautiful arrangements of the nectaries of this flower. In the following summer observation showed me that it was fertilised by bees. At the time I thought I fully understood how this came about. Now, however, I found that I had erred in regard to the last point, because then I still believed all hermaphrodite flowers to be fertilised by their own pollen.

"Finally, last summer I studied the common spurge (*Euphorbia Cyparissias*) and found in it an arrangement the exact opposite of that described above, the flower being fertilised by insects, but in such a way that they carry the pollen of older flowers to the stigmas of younger ones.

"Upon these six chief discoveries, made in five years, is founded my 'Theory of Flowers.'"

After a discussion of the subject of nectar and its uses, in which he demolishes the older views, such as that the nectar causes the growth of the ovule to a seed by keeping it damp, or that it is an injurious substance, whose removal by insects is therefore a direct benefit to the plant, Sprengel goes on to point out how in all honey flowers the following five points may be observed, viz.:—(1) The honey gland or *nectary*; (2) the *honey receptacle*, in which is stored the honey secreted by the gland; (3) some arrangement to protect the

[3] The flower is *dichogamous, i.e.*, the stamens and pistil do not ripen simultaneously. In young flowers the stamens are ripe, the style folded back out of the way; in older ones the style occupies, with its ripe stigmas, the place formerly held by the now empty and withered stamens. This phenomenon is termed protandrous dichogamy or protandry; the reverse case (*Euphorbia*) protogyny.

honey from the rain; (4) arrangements to enable insects easily to find the honey, such as size and colour of the corolla, scent, and the honey-guides or "path-finders" formed by the differently-coloured spots near the entrance to the honey; (5) the general impossibility of spontaneous self-fertilisation, owing largely to dichogamy or other arrangements, and the necessity of insect aid.

The book contains an enormous mass of the most painstaking observations on numerous flowers, undertaken with these ideas in view. There is, however, one very serious blemish in it, which, perhaps, as much as anything, caused its rejection by botanists. Sprengel was most careful to find reasons for everything in the structure of the flower, and yet he did not try to give a reason for flowers being adapted to fertilisation by insects. Why should a flower go to the trouble and expense of attracting insects merely to effect fertilisation, which it might do for itself very easily? Sprengel observed that by dichogamy and other arrangements it constantly occurred that a flower was fertilised, not with its own pollen, but with pollen from a different flower. He even went so far as to say, "Since very many flowers are unisexual, and apparently at least as many of the hermaphrodites are dichogamous, Nature seems to have intended that no flower should be fertilised by its own pollen," and yet he did not suspect that this was the whole point of the fertilisation by insect agency. Darwin and others have shown the great benefits arising from cross fertilisation, and have thus explained why plants should have become adapted to fertilisation by insects in the various ways described by Sprengel. Had Sprengel been aware of this point, and incorporated it in his theory of flowers, it seems unlikely that his work could have fallen into the oblivion which soon overtook it. Even so late as 1850, the work could be bought for 1s. 6d. (it is now quoted by Friedländer at 22s.). In 1841 it came into the hands of Charles Darwin. "The book impressed him as being 'full of truth,' although with 'some little nonsense.' It not only encouraged him in kindred speculation, but guided him in his work, for in 1844 he speaks of verifying Sprengel's observations" (Life, by Francis Darwin). He rehabilitated Sprengel by his biological work, especially the "Origin of Species," the "Cross and Self-Fertilisation of Plants," where he recounted the results of a long series of experiments showing the benefits of cross fertilisation, and, lastly, by his brilliant work on "Fertilisation of Orchids," a book of observations on flowers, conducted much on Sprengel's lines, but with the flaws of his theory removed. Since this time, the whole subject of fertilisation, by insects and otherwise, has received much attention at the hands especially of Hildebrand, Axell, Delpino, Hermann and Fritz Müller, and recently Macleod and Robertson. To the writings of these authors, reference must be made for further information, and we must here take our leave also of Sprengel, a man deserving of a high place in the History of Botany, but most unjustly forgotten for nearly seventy years after the publication of his classic book.

John C. Willis.

III.

The Recapitulation Theory in Palæontology.

NATURAL SCIENCE is to be congratulated on the publication of an article so opposed to current belief as that of Mr. C. Herbert Hurst on "The Recapitulation Theory," for it has thereby shown that it will not burke views simply because they are unfashionable, but rather that it is ready to afford a free field to all genuine knights-errant who dare to smite the shield of authority. Whether the heterodox opinions prove ultimately right or wrong, their publication is of service as forcing us to consider more carefully than we are apt to do the reasons for the faith that is in us. No doubt there will be many to answer Mr. Hurst's challenge, some, perhaps, to support him; out of the *mêlée*, truth is most likely to arise if each confines himself to facts within his own knowledge; here are a few such.

The heaviest blows of Mr. Hurst fall on the embryologists, or, to speak more accurately, on those who study the embryology of living beings, "without the labour involved in palæontological research." Such an attack is undoubtedly deserved in many cases; but Mr. Hurst himself would have strengthened his position had he been able to bring forward any arguments from the actual history of extinct beings that should upset the conclusions of the neontologists, or that should definitely disprove the dictum—"Ontogeny repeats Phylogeny." This he has not done, and this I do not intend to do for him. In the first place, though it would be easy to show that the genealogies constructed by neontologists were contradictory both of one another and of the facts of palæontology,[1] this would merely prove that somebody had made mistakes, an argument admirably adapted for the daily Press, but not for a scientific journal. In the second place, the very limited amount of accurate palæontological knowledge that I possess does not enable me to produce any facts opposed to the theory of Recapitulation as understood by most modern biologists.

First, let us consider the case of *Antedon*, which Mr. Hurst dismisses so scornfully. The possession of a stem is, we may admit, an advantage to the larva, and Mr. Hurst's contention that the larval

[1] See a recent article by A. Smith Woodward on "The Forerunners of the Backboned Animals," NATURAL SCIENCE, vol. i., p. 596.

stem of *Antedon* is the equivalent of the *larval* stem of *Antedon's* ancestors, would be at least a possible explanation, were it not for one fact: the stem-ossicles of the *Antedon* larva are not at all of a simple type, they have a very peculiar specialised structure, and, broadly speaking, resemble the stem-ossicles of the Bourgueticrinidæ so common in the Upper Chalk. It is a mistake to call this larva "pentacrinoid": if any genus is known to us with which it may be compared, that genus is *Thiollericrinus*, of the Jurassic rocks, many species of which show a gradual loss of the stem and development of cirri around the base of the cup. What the ancestors of *Thiollericrinus* may have been we cannot say with certainty; this only seems clear, that the structure of the stem is specialised. A somewhat similar structure was developed in the Carboniferous Platycrinidæ, from which, however, it is not likely that *Thiollericrinus* was derived. Such stems do not occur lower than the Carboniferous, and we must infer that the ancestors of both Platycrinidæ and Bourgueticrinidæ had stems of the primitive Palæozoic type, with round ossicles radiately ridged. From such ossicles all other types may easily have been derived. Now, to accord with Mr. Hurst's explanation of the *Antedon* stem, it must be supposed, either that the larval stem of all crinoids has always been of this specialised type, which is contrary to all available evidence, or that its structure is a special larval development. Surely it is hardly probable that an organ which is only in use for a few days of the creature's life should have become specialised in just the same way as the stem of the adult ancestors. It is certainly easier for the ordinary mind to imagine that, by acceleration of development (a principle ignored by Mr. Hurst), the structure of the adult ancestor has been pushed back to the larval stages of the existing *Antedon*.

But the stem is by no means the only structure in the larval *Antedon* that bears on the Recapitulation Theory. "Each transient stage in the development of an individual," says Mr. Hurst, "is a modification of the corresponding stage of development of its ancestors." Let us see how this is borne out by the development of the cup-plates.

The most important peculiarity of the larva as distinguished from the adult *Antedon* is the presence of a plate in the anal interradius. At an early period this plate appears between two of the radials, and on the same level with them; subsequently it is lifted out from between the radials, which close in under it as it passes upwards, so that at last the radial circlet is quite continuous, and the anal plate lies above it. At about this stage the crown is separated from the stem; shortly afterwards, the orals disappear, and a little later, the anal plate is itself absorbed and vanishes. Now, on Mr. Hurst's theory, each of these stages should be represented in the early development of the ancestors of *Antedon*; but this would raise far greater difficulties, for the oldest crinoids do not possess this anal plate at all, at all

events not in the position that it first occupies in *Antedon*. It appears from palæontological evidence that this plate first appeared above the level of the radials, that it gradually sank down between the two posterior radials, and that at a far later period, towards the close of the Palæozoic, it gradually passed upwards again, precisely as it does in the young *Antedon*, and eventually disappeared. The ontogenetic stages of the anal plate in *Antedon* are represented in the phylogenetic series by *Ceriocrinus*, *Erisocrinus* and *Stemmatocrinus*. Mr. Hurst is bound to suppose that such forms as *Merocrinus* and *Dendrocrinus* started with an anal plate in a line with the radials, a supposition contrary to all available evidence, or that the anal plate in *Antedon*, together with the remarkable changes that it passes through, is a special larval development. "Mystical" it may be, to suppose that we have here an epitome of the ancestral history, but Mr. Hurst can hardly say that it is not justified by evidence.

There are many other curious parallelisms between the growth of *Antedon* and the history of the earlier crinoids. It is, however, possible to explain them on Mr. Hurst's plan, so I merely allude to them here to show that they do not in any way run counter to the Recapitulation Theory. They are the large size of the basals, the peculiar shape of the radials, the well-developed orals, and the evolution of pinnules.

To turn to another passage and to a different group of animals. At the bottom of page 197 Mr. Hurst says: "In order to produce a 'record,' it is necessary that new chapters be added at the end of the pre-existing record. It is necessary that, as the adult structure varies in one direction, the late stages of development shall vary in another, so as to become, not more like the new adult structure than they were before, but more like the old one." This is hardly a fair statement of the case. It is not correct to say that the late stages of development must vary in *another* direction; for it surely is the case, in any series of parents and offspring, which are varying in a given direction, and which we may denote A^1, A^2, A^3, A^n, that A^6 is nearer to A^7 than A^5 is. Consequently, if the latest stage of development of the form A^7 resembles A^6, it is necessarily more like A^7 than a stage which resembled A^5. That is to say, on the Recapitulation Theory, the stages of development vary in the *same* direction as the adults. Mr. Hurst's statement of the method of variation, demanded by the Recapitulation Theory, should, therefore, be amended as follows:—Variation, or change from parent to offspring, takes place by the addition of features at the end of the ontogeny; and these features are, by subsequent successive additions, gradually pushed back to earlier stages of ontogeny, so that what is the ultimate stage of one form is the penultimate of the next, and the ante-penultimate of the next after that.

Although the adherents of the Recapitulation Theory will doubtless accept the above as, in the main, a correct statement of the

method of development, yet not the most sanguine of them will hope to find so perfect an epitome of phylogeny in the majority of cases. The necessary compression, the constant elimination of unnecessary stages, the modifications required by larval conditions—all these things make it truly remarkable that we should get as much recapitulation as we do. Yes, we do get it, and this is "the way in which it does actually occur," let Mr. Hurst deny it as much as he pleases. In fact, no more perfect example of this impossible and non-existent method of development could be imagined than that which is actually afforded us by the palæontological history of the Ammonites. Does Mr. Hurst dispute the reality of the facts made known to us by Würtenberger, Waagen, Branco, Hyatt, Buckman, and many others? Not everyone will accept the alterations in classification proposed by these workers, but no one has hitherto been found to deny their facts. The reason why no one has done so is obvious: anyone can verify them for himself. It has been shown over and over again, by workers approaching the subject quite independently and from different points of view, that the adolescent stage of any species of Ammonite resembles the adult stage of its immediate ancestor, and that the larval stage resembles the adult of a previous ancestor; while, on the other hand, the senile stages of the same species foreshadow the features of the adult offspring. These facts have not merely been worked out by students in museums, by dissection of well-preserved individuals, and comparison of large quantities of specimens, but have been confirmed by minute labours in the rocks themselves, and by the careful tracing of the species from zone to zone over large tracts of country.

A suitable example is presented by the descent of *Coroniceras trigonatum* from a smooth ancestor, as indicated in Hyatt's "Genesis of the Arietidæ," Summary-plate xii. Hyatt here gives the ancestry as follows:—*C. trigonatum*, *C. gmuendense*, *C. lyra*, *C. rotiforme*, *C. sauzeanum*, *C. kridion*, *Arnioceras kridioides*, *A. semicostatum*, and *Psiloceras planorbe*, var. *leve*. To obtain an independent opinion, I applied to my friend, Mr. S. S. Buckman, who informs me that he agrees with the first six names except as to the interposition of *C. sauzeanum*. He, however, "would not like to say that the species of *Arnioceras* are the actual ancestors; but they are the morphological equivalents undoubtedly of those ancestors." There is also considerable objection to taking a retrogressive type like *Psiloceras planorbe* as the starting point of a new series. At the same time, even those who refuse to regard this particular species in the same light as Professor Hyatt, will admit that the ancestor must have been a somewhat similar smooth, keelless, and round-sectioned form; in fact, Buckman favours *Arnioceras miserabile*, which Hyatt himself gives as an ancestral form. Accepting, then, this line of descent, we may draw up the accompanying table, which traces the development of three main characters in both ontogeny and phylogeny. The first column describes the infantine stage, the second the adolescent, and the

third the adult of each species. In none of these is the embryonic stage given, since it varies very little, being of the same smooth character throughout. It becomes, therefore, interesting to learn how Mr. Hurst explains the tuberculate infantine stage of *C. trigonatum*, which is certainly considerably less like its own adult than is the corresponding stage of *C. lyra;* it cannot possibly be explained as an attempt of the embryo to become like the adult. Indeed the whole table, the facts of which are given in full detail in Hyatt's monograph, and the main principle of which is maintained by two independent workers, is totally opposed to all Mr. Hurst's contentions. It should also be remembered that there are many details of structure, too complicated to be dwelt on here, but all confirming the other evidence.

	Infantine.	Adolescent.	Adult.
Coroniceras trigonatum	Well-marked ribs and slight tubercles.	Broad ribs.	Nearly smooth.
	Quadrate section.	Section unknown.	Subtriangular section.
	Keel?	Slight keel.	Very slight keel.
C. gmuendense ...	Well-marked ribs and slight tubercles.	Clear ribs and slight tubercles.	Slight ribs.
	Subquadrate section.	Compressed quadrate.	Compressed quadrate.
	Slight keel.	Keel.	Pronounced keel.
C. lyra	Ribs and slight tubercles.	Ribs and tubercles.	Tubercles.
	Broad subquadrate section.	Subquadrate section.	Slightly compressed subquadrate section.
	Slight keel.	Slight keel.	Keel.
C. rotiforme	Smooth, and then slight ribs.	Ribs and tubercles.	Pronounced tubercles.
	Roundish section.	Broad subquadrate.	Subquadrate section.
	Very slight keel.	Slight keel.	Keel.
C. kridion...	Almost smooth.	Slight ribs.	Tubercles.
	Broad rounded section.	Broad subquadrate section.	Subquadrate section.
	No keel.	Slight keel.	Keel.
Arnioceras semicostatum	Smooth.	Smooth and slight folds.	Slight ribs.
	Roundish section.	Elliptical section.	Slightly broader section.
	No keel.	Trace of keel.	Slight keel.
A. miserabile.	Smooth.	Smooth.	Smooth.
	Round section.	Slightly compressed round section.	Elliptical section.
	No keel.	No keel.	Slight keel.

Professor Huxley once said that if Evolution had not already been an accepted theory, the palæontologists would have had to have invented it. It might, with no less truth, be asserted that, if the embryologists had not forestalled them, the palæontologists would have had to invent the theory of Recapitulation. As a matter of fact, many of them seem actually to have arrived at it quite independently either of one another or of the neontologists, a circumstance that does not necessarily prove the truth of the theory, but that should, at least, have led Mr. Hurst to pay some attention to their opinions. With evidence such as has just been quoted before them, it would, indeed, have been difficult to have arrived at other conclusions; at the same time, it must not be supposed that all lines of descent, even among Ammonites, show the facts with equal clearness. There are, as has already been hinted, many modifying forces at work, and the chief of these may be conveniently included under the one head of economy. To this is due the disappearance from the record of those features that are not adapted to the needs of the animal in the early

stages of its life, and of those that involve unnecessary expenditure, such as a spinous stage in an Ammonite intervening between two costate stages. These explanations of the falsification of the epitome are barely alluded to by Mr. Hurst; for since, on *à priori* grounds, he denies any epitome whatsoever, he naturally does not stay to enquire whether he may not have sometimes been misled by these obscurations of it. So far, then, as Mr. Hurst is concerned, it is idle to discuss these matters, we must rather seek for flaws in his original argument.

His main fallacy appears to me to be simply this, that he substitutes contemporary relations for ancestors. After stating Von Baer's law, which obviously refers to existing species, co-existing, that is, and therefore not derived the one from the other, he proceeds to say: —"If similar comparisons could be instituted between an ancestral species and its much modified descendants, there is no reason for doubting that a similar result would be reached." But there is all the difference in the world between filial and fraternal relationship, and though Von Baer's law is undoubtedly true of the latter, there are many objections to supposing that it is equally true of the former. The only evidence that Mr. Hurst condescends to offer is the quotation from Darwin, which is, indeed, one part in favour of himself, but the other six parts in favour of his opponents.

Mr. Hurst seems to suppose that on the Recapitulation Theory a bird should begin life as a fish, then change to a lizard or other reptile, and finally burst into a bird. Such a harlequinade has never been imagined. The bird of to-day must be compared, not with the reptile of to-day, but with the bird of the past, and that in its turn with a form which may have had very few of the characters of reptiles as we now know them; as for fish, it is as likely as not that they barely entered into the phylogeny at all. Those who keep a more observant eye on the progress of vertebrate palæontology than I have leisure to do must surely have observed how day by day the ancestral stocks are pushed further and further back, so that the connecting link between, or common ancestor of, the great divisions cannot possibly be dragged into the discussion.

To glance at another example of Mr. Hurst's—the three gnats *Culex*, *Corethra*, and *Chironomus*, forms which, though alike in the adult, are dissimilar in the larval stage. Of course, it is possible that these genera may really be descended from a common ancestor, and that variation has chiefly affected the early stages. If so, it is clearly impossible that those early stages can reveal to us the past history of the genera in question. Such cases as this are admitted by everyone; they are consistent with the Recapitulation Theory, and as Mr. Hurst makes no further capital out of them, it is hard to see why he introduced them at all. On the other hand, instances are known, as in the Ammonites *Dumortieria* and *Grammoceras*, of adults which resemble one another so closely that they would actually be taken for

the same species, did not investigation of their ontogeny reveal stages resembling the adults of pre-existing species belonging to the two different genera. In these cases, it is the earliest stages that differ most and afford the best proof not only as to their affinities with contemporary species, but as to their perished ancestry. It would be rash for one who is no entomologist to hint that the resemblance of the adult *Culex*, *Corethra*, and *Chironomus* is a similar case of convergence; but it is clear in the case of *Dumortieria* and *Grammoceras* that "variation" apparently "affecting chiefly the structure of the individual in early stages of development" has actually preserved to us a record of the ontogeny.

Another mistake of Mr. Hurst, is his refusal to admit more than one method of variation. He appears to deny that variation occurs at the close of ontogeny; if an adult structure varies, he says in effect, it ceases to exist as an adult structure at all. It does eventually do so, no doubt, but not immediately. The very same "change in the constitution of successive generations of a species leading to the production of a new species" does take place in the life-history of a single generation. Ontogeny, in fact, forecasts phylogeny just as much as it repeats it. An Ammonite that is smooth in the adult, will become ribbed in old age; this change takes place at earlier and earlier periods in successive generations, till at last a form is attained that has ribs in the adult. Then we call it a new species. But this new species is spinous in its old age; gradually do the spines, in like manner, appear sooner and sooner, till another species, that bears spines in its maturity, is found in the rocks. A similar case is the tendency of modern civilised man to adopt baldness as an adult character, although one that has hitherto been regarded as purely senile. Yet all these well-known instances of variation (and I use the term precisely in Mr. Hurst's own sense) "occur in a way utterly unlike the way in which it does actually occur," *i.e.*, within Mr. Hurst's horizon. Of course, this is not the only method of variation; there are sports of differing degrees, and there is variation produced by sexual mixture of different strains. If these alone were to be considered, we might, of course, be led to different conclusions, that is, if we were persistently to shut our eyes to the plain facts of palæontology.

The fact that absurdities and contradictions have arisen from too zealous following of the Recapitulation Theory does not actually prove the conception false; nor, on the other hand, does the fact that under its guidance we have made remarkable discoveries and initiated fruitful investigations necessarily prove its truth; at the same time, it might possibly lead Mr. Hurst to reconsider his condemnation if he were to make himself acquainted with the brilliant researches of Hyatt and Buckman on Cephalopoda, R. T. Jackson on Pelecypoda, and Beecher on Brachiopoda, not to mention many writings in other tongues than our own, all which works have been inspired by the Recapitulation Theory.

F. A. Bather.

IV.

Ornithology in Relation to Agriculture and Horticulture.[1]

THE subject of agricultural and horticultural economics, considered in the relations of birds and insects to the produce of the soil, is one which, till recent years, has been singularly neglected in this country. In France, Belgium, and especially in America, as well as in other civilised countries, the most careful and practical investigations, under the assistance of the State, have led to the accumulation and diffusion of much valuable information on this and kindred subjects. In England, the researches of Miss Eleanor A. Ormerod (late consulting Entomologist to the Royal Agricultural Society of England) are recognised as of great value in enabling the farmer and gardener to detect the various insect pests which take tithe of his crops, and in teaching him how best to apply suitable remedies for their prevention or extirpation.

In the present volume the editor, Mr. John Watson, has brought together a series of useful papers and notes by various naturalists, whose names are a sufficient guarantee of the high practical value of the opinions expressed, in connection with ornithology in its bearings on agriculture and horticulture. On taking up the book we are somewhat disappointed to find that it contains neither preface nor introduction stating under what circumstances the various papers were written, we can only conclude, therefore, that these have already appeared either as newspaper articles or in some serial. The book is divided into twelve chapters, under the various headings of Hawks and Falcons, Owls, Wood-Pigeon, Rook, Starling, Miscellaneous Small Birds, Game Birds, and an appendix, with notes and additions. No less than five chapters are devoted to the sparrow, those "rats of the air," "ruffians in feathers," whose mischievous and destructive character are recognised and acknowledged by farmers and gardeners over half the world.

The larger birds of prey, buzzards, kites, goshawks, harriers, and the noble peregrine are now virtually extirpated in England, and

[1] Ornithology in Relation to Agriculture and Horticulture. By various Writers. Edited by John Watson, F.L.S., &c. London: W. H. Allen & Co., 1893.

the smaller hawks and falcons, and also owls, are far less numerous than formerly. There are at present large districts in England where you may wander all day without seeing a single bird of prey. This has been brought about by the excessive rage for game-preserving. As a class, gamekeepers, considering the great opportunities at their disposal, are proverbially ignorant of Natural History, and seem quite incapable or unwilling to discriminate, even from their own narrow standpoint, between the good and the bad. The natural result, therefore, of so much misplaced zeal has been an enormous increase in wood-pigeons, sparrows, rats, and mice, which, now that their natural enemies, the birds of prey and weasels, have been destroyed, flourish and multiply unchecked, and yearly destroy great quantities of valuable cereals and other farm and garden produce.

So much has already been written in defence of the farmers' best friends, the owls, that any further allusion to the subject would only be hackneyed and out of place. We will only, therefore, mention one fresh fact in proof of the value of the owl as a vermin-destroyer. In the vole-plague districts of the south of Scotland, which includes a wide area of hill-pasture in Teviot and Hawick, Ettrick, Eskdalemuir, Yarrow, and Moffat, in 1892, 301 nests of the short-eared owl (*Asio accipitrinus*) were actually found on those farms from which specific information was obtained, and it is calculated that this number may be reasonably doubled to include those not seen.

The result is 602 nests, with, say, seven young to each nest, equal to 4,214 young birds on these farms. These owls have undoubtedly been attracted to the district from great distances by the enormous supply of their natural food, and are induced to remain and nest there, and the services rendered by them to the sheep farmers cannot well be estimated. To give three instances alone, twenty-nine dead voles were taken from the side of one nest, and the next day twenty-seven from the same place. In another case, a shepherd counted thirty-seven at a nest containing ten eggs. For some years past the short-eared owls are known to have nested in small numbers in the infected area, but in 1892, owing to the abundance of food, they have mustered, remained, and bred in extraordinary numbers, with the result that there has been a marked diminution of the voles over much of the districts named. The mouse-eating kestrel has also greatly increased since the commencement of the plague, and as many as thirty have been seen at one time. The whole question has been most ably treated by Mr. Peter Adair in a paper which appeared in "The Annals of Scottish Natural History" for October, 1892.

In the five chapters relating to the sparrow, the evidence for the prosecution greatly outweighs that for the defence. It is clearly shown that the depredations of this pest on fruit-tree buds, to fruit farmers, florists, young crops of vegetables, and more especially to corn in autumn, is enormous, and far in excess of any benefits con-

ferred by the consumption of injurious seeds and noxious insects. They entail also direct harmful consequences by their pugnacious and self-assertive nature in driving off useful insectivorous birds from the neighbourhood of their haunts. Yet it is by no means clearly proved that an utter and complete extermination of the sparrow-nuisance would be a benefit, for when man upsets the balance of nature, he very often has to pay for it in some form or other.

The sparrow certainly requires no Act of Parliament to protect him, and the plea of sentimentalists and humanitarians that he should be allowed to increase and multiply unchecked, will certainly never be listened to by those country folk who are best able to form a judgment in the matter.

There can be no doubt that, during the last half-century, the woodpigeon or ringdove (*Columba palumbus*) has increased to an enormous extent. The causes of this increase are, doubtless, the killing off of the falcons and hawks, which are the natural enemies of the race, the increase of woods and plantations, especially those of fir, and the abundance of winter food in turnips and other green crops. It is quite certain, too, that in the autumn the ranks of our local birds are greatly increased by immigrants from the Continent. In the autumn, woodpigeons congregate and attack the ripening corn, particularly in those spots where it is storm-laid, devouring great quantities, and crushing and trampling the heads to near the ground, so that in a wet season much becomes hopelessly sprouted. In winter they commit serious ravages in the turnip crops by eating the leaves, thus exposing the bulb to frost. They are also very partial to the young clover plant. The ringdove, however, has redeeming points, its plaintive coo, rōō, coo, coo is a pleasant sound at early morning in spring woodlands, and in the winter it is a real sporting bird, and excellent eating.

The injury done by rooks has often been much exaggerated by farmers and others. If we put aside those periods of the year when it levies contributions on the newly-sown corn, especially when badly covered, the time when the corn is ripening, injury done to stacks and swede turnips in severe weather—we have pretty well enumerated all the charges brought against him. All the rest of the year he is ridding the pastures of injurious grubs, such as the larvæ of the cock-chafer (*Melolantha*), and of the cranefly (*Tipula*). In recent years, rooks, in those districts where they are too many, or short of food in a drought, have been accused of developing egg-stealing propensities, and harrying the nests of game birds and wild fowl, and killing the young, and we are afraid he is not altogether guiltless in this respect.

The starling, considered from an agricultural point, is the greatest possible friend both of farmer and gardener, its food during the whole of the year consisting of grubs, small molluscs, worms, and insects, and only very occasionally fruit and berries. In the autumn immense flights of migrating starlings come to us from the Continent ;

these are an Eastern race, distinct from our common bird, and have purple heads and green ear-coverts, and they leave the country in the spring.

We are somewhat doubtful as to what Mr. Riley Fortune tells us in chap. x., that the starling picks the "ticks" from the wool on the backs of sheep. We rather think he uses the sheep's back as a convenient perch. At various times we have watched them through a glass at a short distance, when perched on the backs of thick-wooled Lincoln sheep, but have never seen them actually picking off the sheep fags.

Much more might well be written on this most interesting topic, the subject is practically an inexhaustible one. Out of the multitude of our miscellaneous small birds, the greater part are absolutely innocuous and are largely beneficial. In other cases, the injury done at certain seasons is slight and amply counterbalanced by the services they render during the rest of the year in destroying insects and the seeds of weeds.

In conclusion, to sum up the evidence both for and against, as placed before us by the able ornithologists in Mr. Watson's book, it is abundantly apparent that the case for the prosecution falls very far short of the defence, and that the verdict must be an acquittal for the birds, both as regarding individual species and in the aggregate, with an admission that the benefit they confer upon man is far in excess of the injury. There is one exception to this, and that is the ubiquitous and all-devouring sparrow.

"O wretched set of sparrows, one and all,"—

Perhaps no greater mischief is done than by that large class of sentimental writers who are ever ready to exaggerate the good qualities of their feathered favourites and to minimise the evil. It must, however, be apparent to the dullest intellect that no wild bird is able to draw a line between the natural production of the soil and those seeds and fruit which are the results of man's industry. Neither can it be expected that hawk or falcon can discriminate between the young of a wild bird and a coop-reared pheasant or partridge. In so highly a cultivated country as England, birds would often be put to great straits if they had to depend on wild fruits or seeds alone, and, having dispossessed them of their inheritance, we must be satisfied to give our little workers some small share of our produce as a return for the important services rendered in keeping down weeds and insects, and thus indirectly helping to increase the fertility and productiveness of the soil.

JOHN CORDEAUX.

V.

A Fish-Eating Rodent.

A VERY interesting new Mammal has recently been received at the British Museum, in the form of a Fish-eating Rat from the mountain streams of Central Peru. The animal is of about the size of a common house-rat, but has a flattened head, strong and numerous whisker-bristles, and very small eyes and ears, characters which give it a striking resemblance in its physiognomy to some of the aquatic genera of the Insectivora and Carnivora, such as *Potamogale*, *Myogale*, *Lutra*, or *Cynogale*. Its swimming powers are evidently very great, as is shown, among other things, by its broad, webbed and strongly-ciliated hind feet, far better developed for this purpose than are those of the ordinary swimming Muridæ, such as our English Water-vole, whose simple vegetarian diet does not necessitate the development of any exceptional swimming powers. In colour, like the common Water Shrew, it has a dark upper side with a whitish belly, and has a markedly bicolor black and white tail.

The chief interest of the new form centres in the fact of its being wholly a fish-eater, and in its having in connection therewith its incisor teeth modified for catching a slippery, active prey by the development of their outer corners into long sharp points, and its intestines altered by the reduction almost to *nil* of its cœcum, an organ in vegetarian Muridæ always of great size and capacity. The stomach of the single specimen obtained contains fish-scales, recognised by Mr. Boulenger as those of *Tetragonopterus alosa*, a fish whose average length is about six inches.

This animal represents quite a new departure in Rodent life-history, for although it is now perfectly well known that the North American Musquash (*Fiber zibethicus*) occasionally feeds on fish caught by itself, yet there is no other Rodent which, as in the case of *Ichthyomys stolzmanni*, as it is proposed to term the new form, wholly lives on fish, to the exclusion of a vegetable diet.

The general relationships of *Ichthyomys* are clearly with the ordinary South American Muridæ, perhaps more especially with those of the *Habrothrix* group, and there is certainly no direct connection with *Fiber*.

The type and only known specimen of this interesting form was obtained by the Polish collector, Mr. J. Kalinowski, at Chanchamayo, Central Peru, in the course of 1891.

OLDFIELD THOMAS.

VI.

Colour Changes in Insects.

THE question of the variation in colour and markings exhibited by many species of lepidoptera, in both their preparatory and perfect stages, has, during the last few years, received much attention, and given rise to not a little controversy. A general protective resemblance of insects to their surroundings has long been an accepted fact with most naturalists, and no one would deny that the special and often highly perfect likeness of caterpillars to twigs and imagines to leaves, is of value as a protection; but on the meaning of the variation in colour of a species with reference to its surroundings, less certainty has existed. This most interesting subject was brought into prominent and public notice in 1890, by Poulton, in his well-known book, "The Colours of Animals" (pp. 110–158). Some doubt has lately been expressed as to certain of his deductions. His recent publication (1) of the details of several years' experiments, of which only some of the leading results were given in "The Colours of Animals," is, therefore, of value, and a short review of the questions raised may be of interest.

The only larvæ of Noctuid moths on which Poulton experimented were those of *Hadena oleracea*, *Euplexia lucipara*, two species of *Mamestra* (*M. brassicæ* and *M. persicariæ*), and four of *Catocala* (*C. sponsa*, *C. nupta*, *C. fraxini*, and *C. elocata*). The *Mamestræ* showed no power of colour-adaptation, some green caterpillars of *M. persicariæ* turning brown on green leaves. Most noctuid larvæ feed by night, hiding in earth by day, and colour is consequently of minor importance to them. The *Catocala* larvæ, which in their form and habits approach geometers, were rather sensitive to environment, tending to become darker when black twigs were mixed with their food, than when among green leaves and shoots only. Poulton's results with these two genera are confirmed by Miss Gould (2). No certain results were obtained with the larvæ of *Hadena* and *Euplexia*.

The only geometrid caterpillar which was not proved sensitive to its environment was that of *Ephyra annulata* (*omicronaria*). All others on which experiments were made were darkened by placing black twigs among their food. Such were the larvæ of *Ennomos quercinaria* (*angularia*), *Selenia lunaria*, *Melanippe montanata*, *Phigalia pedaria* (*pilosaria*), *Hemerophylla abruptaria*, and *Crocallis elinguaria*. All these were light brown among green leaves and shoots. The last-named caterpillars were

placed among green leaves in darkness, and were darkened thereby, though not nearly so strongly as by dark surroundings in daylight. The larvæ of *Geometra papilionaria*, which hibernate, were sensitive before the winter, when various shades of brown in colour; in the spring, however, when they become dimorphic, brown or green, they were found to be no longer sensitive. The caterpillars of *Boarmia roboraria* were, before hibernation, dark grey or brown in a dark environment, and greenish among green leaves.

Still more satisfactory results were obtained with caterpillars of *Rumia cratægata*. These were dark brown among black twigs, and light brown with green patches among green leaves. Miss Gould (2) also obtained similar results with this species. A striking experiment by Poulton in the subsequent year seemed to show that there is no tendency of these acquired colours to be inherited; indeed, the caterpillars from eggs laid by moths reared from the darker caterpillars were more easily affected by green surroundings than by dark; while the offspring of moths reared from the lighter larvæ were more sensitive to dark surroundings than to green.

But the most remarkable results were given by caterpillars of *Amphidasys betularia*. These were nearly all dark when many dark twigs were placed with the food, mostly dark when some twigs were inserted, and all green when no twigs were used. The presence of white paper spills caused the larvæ to assume a remarkable whitish hue. In 1892, Poulton carried out a more detailed series of experiments on this species. Caterpillars among black twigs were found to be nearly or quite black, those among brown twigs were brown, those among dead leaves were mottled-brown with vein-like markings. Artificial dark surroundings (black paper or enamel) caused the larvæ to be dark, but were not so effective as the twigs. Dark objects not in contact with the larvæ (piled around the cylinders containing the food) had no effect. Among green leaves and shoots only, the larvæ always turned green (the early stages are always brown), except when they were much crowded, in which case they had rather a darkening effect on each other; it appears that dark surroundings act more readily than green. With dark and green environment in feeble light, similar results were obtained to some extent, but the differences were by no means so marked as in daylight. In darkness, no difference was produced, whatever the surroundings, the caterpillars being always black, brown, or grey of various shades. The presence of blue paper spills caused the larvæ to assume a dark purplish hue, while orange paper among the food leaves produced a green colour in the caterpillars.

Poulton has shown that the dark pigments (to which the various shades of brown, black, grey, etc., are due) are deposited in the cells of the epidermis, while the green colouring-matter is found in the subjacent fat. The presence or absence of both sorts of pigment is determined by the surrounding objects, through some quality in the

light reflected from them. The suppression of the superficial dark pigment thus leads to the green colour of the caterpillar.

From these experiments, the fact that geometrid larvæ have a very considerable power of colour-adaptation to their environment appears to be beyond dispute. There seems no reason to doubt that this power is of considerable protective value to the insects, especially when we consider that the larvæ in which it is developed are all of a form closely resembling the twigs of their food-plants, but that they need some power of colour-adaptation to render the resemblance perfect. Poulton lays stress on the fact that all the numerous varieties of *A. betularia* produced by the experiments would be in correspondence with some possible natural environment. The experiment of moving caterpillars from one set of conditions to another showed that the early larval stages only are susceptible to any great extent; after the last moult but one, little or no change was produced. Hence, the most sensitive caterpillar has not that power of changing and rechanging its hues which is possessed by many fishes and reptiles; but then, in natural conditions, the environment of caterpillars will very rarely be changed.

In "The Colours of Animals," Poulton gave several instances of variation in the colour of the cocoons spun by caterpillars before pupation, which appeared to correspond to changes in the environment. The cocoons of *Saturnia pavonia* (*carpini*), *Eriogaster lanestris*, and *Halias prasinana* were found to be brown among leaves, and white in paper bags. Bateson (3, 4) has, however, now shown that the white cocoons of the two former species are not due to the colour of the surroundings, but to the disturbance of the larvæ before spinning. Caterpillars which were removed from their food, when ready to spin, produced white cocoons whether placed in dark or light bags, while when white strips of paper were placed about the food, without disturbing the larvæ, they spun dark cocoons. Bateson has further established that the dark hue of the cocoons is due to a brown fluid contained in the alimentary canal, with which the caterpillar colours the silk as it is discharged from the mouth, perhaps, also, staining the finished cocoon by an ejection from the intestine. When a caterpillar is disturbed, this fluid is voided before spinning, and hence the cocoon is white. Tutt has discovered (5) a similar cause for the varying colours of the cocoons of *Halias chlorana*, which, however, do vary in hue with their surroundings to some extent. Poulton has now made further experiments on *H. prasinana*, and, carefully avoiding disturbing the larvæ, finds that the cocoons of this species do vary in accord with the colour of the surroundings. The caterpillars seem to exercise choice in the colour of the silk they produce, and to be less irritable than those of *Saturnia pavonia* and *Eriogaster lanestris*. A larva which had begun to spin a white cocoon in a chip box was removed, an attempt was made to cut out the egg of an ichneumon, and the caterpillar was injured thereby.

Nevertheless, this insect, when placed among oak leaves and twigs, spun a dark cocoon! Such difference in behaviour in different species should guard observers in future against too hasty generalisation.

Poulton's recent experiments on butterfly pupæ confirm the results given in "The Colours of Animals." The most striking effects were obtained with *Vanessa urticæ* and *V. io*. These chrysalids vary from dark brown or black to bright golden (*urticæ*) or metallic green (*io*). As in the larvæ, the latter effects are due to the absence of dark superficial pigment, and are produced by light, or bright metallic surrounding surfaces. As with the larvæ, dark surfaces lead to the formation of the dark pigment. The pupal colour is fixed in *V. urticæ* by the surroundings of the larva when in the early stages preparatory to pupation, wandering in search of a suitable spot for attachment, or resting motionless before attachment. These stages are lengthened by dark surroundings, and also by disturbance; Poulton believes that during them the "colourless precursor" of the dark pigment which will appear in the pupa is being formed. The last stage of *V. urticæ*, after the caterpillar has suspended itself, is not sensitive, and little or no effect is produced by changing the surroundings then; but in *V. io* this stage is longer, and some result can be obtained by transferring the insects to a different environment.

Caterpillars of *V. urticæ* placed in a gilt cylinder in darkness changed to dark pupæ. Dark surroundings, in a strong light, produced slightly greater darkening of the pupæ than the same environment in the dark. Some larvæ were allowed to pupate in boxes lined with alternate strips of black and gold paper, so that the insect in its motionless stage rested partly on black and partly on gold. These produced pupæ of an intermediate tint, but none were parti-coloured. As the position of the head made no difference in the result, it seems certain that the colour seen by the caterpillar has nothing to do with the effect.

Many experiments besides simple dark and bright surroundings were tried with *V. io*. The larvæ, when exposed to blue light, produced darkish pupæ; when placed on orange and yellow backgrounds, bright green pupæ. Chrysalids of intermediate tints were formed amid bright green surroundings. Dark green surroundings and red paper backgrounds led to dark pupæ, but light falling through green glass, or through red glass or gelatine, made the pupæ light or bright green. When the pupæ were exposed to black ventrally, and to white dorsally, or *vice versâ*, the effect of the dark surface was stronger than that of the white.

The nature of the light-rays reflected from or transmitted through the various substances used, was tested by careful spectral analysis, and it appears that the presence of yellow and orange rays checks the formation of dark pigment, and tends to produce light, green, or bright larvæ and pupæ. The difference between the results with green

leaves and with certain green pigments is thus explicable; the light reflected from the leaves possesses yellow rays, and, consequently, produces light effects on the insects, while the green pigments absorb these rays. So also the red paper backgrounds absorb the rays and produce dark effects, but red glass or gelatine transmits the rays and causes green or light colour in the insects. Petersen (6) seems to have independently arrived at similar results, and to have given the same explanation of them.

The only imagine made the subject of experiment by Poulton was *Gnophos obscuraria*. This moth is mentioned in "The Colours of Animals" as being commonly light on chalk and dark on peat. A number of these insects were reared from the egg, some amid dark, others amid light surroundings, but in no stage was the colour of the insect affected by its environment, and the moths all turned out a light-grey hue. Poulton is led, therefore, to suggest that the prevalence of varieties in nature appropriate to the soil where they occur is the result of natural selection.

The cause of the darkening of moths in various localities has been for several years past frequently discussed, and numerous theories have been propounded. Lepidopterists have noticed that British moths are, as a rule, darker than specimens from the Continent, and that, within the British isles, western, northern, and mountain varieties are darker than insects from the southern and eastern lowlands, the melanic tendency being most strongly marked on the west coast of Ireland, and in the northern and western islands and highlands of Scotland. Yorkshire naturalists have noticed also a special tendency to melanism near the large manufacturing towns of the north, and the dark varieties are said to be increasing, while the lighter types are dying out. It has been suggested that the darkening is, in this case, due to the soot which the unhappy larvæ are compelled to eat with their food!

Natural selection, moisture, cold, and the absence of sunlight, have been put forward as serious suggestions for the cause of melanism. The districts where the phenomenon is most marked are the wettest in the country, and it is evident that constant moisture tends to render dark most objects on which insects rest, and so to favour dark varieties. Natural selection would therefore tend to the preservation of melanic moths in moist districts, but the general impression is that something in moist districts tends also to the production of such varieties. Tutt (7) and many other naturalists, consider moisture of itself to be a cause of melanism. Merrifield believes low temperature to be effective in the same direction, and has, during the last few years, conducted numerous experiments on the effect of temperature in producing dark colouration (8, 9) analogous to the well-known experiments of Weismann. By cooling the pupæ of *Selenia illustraria* (summer brood) and *Ennomos autumnaria*, he has obtained striking results, the moths being rendered decidedly

dark, those of the former species approximating to insects of the spring brood, which are normally dark. He has also succeeded in producing very dark examples of *Lasiocampa quercus*, by cooling the pupæ, and very light specimens of its northern dark variety, *callunæ*, by heating the pupæ (10). In these and other species, there seems no reason to doubt that low temperature applied to the pupæ does induce darkening of the imagine. On the other hand, the upholders of the moisture theory deny that there is such a general darkening of lepidoptera in arctic latitudes as should exist if low temperature be the main cause of melanism, and insist that the climate of the districts where melanism prevails is often mild and equable. This is true, but the summer temperature of these regions is lower than that of southern England, and much lower than that of Continental Europe. It is very possible that moisture may be a true cause of darkening; careful experiment might go far to settle the matter. It seems clear, however, that moisture or cold, or both, tend to the production of melanic varieties, and that these varieties, when produced, are specially favoured by natural selection.

Such observations have been made on few insects except Lepidoptera. There are, however, instances of melanism in Coleoptera. My friend, Mr. H. K. Gore Cuthbert, informs me that in Irish specimens of the beetles, *Badister bipustulatus*, *Dromius quadrinotatus*, *Nebria complanata*, and *Agabus guttatus*, the pale spots and markings are generally smaller than in English examples. A small ground beetle, *Calathus melanocephalus*, with red pronotum, has, in mountainous regions, the pronotum clouded with black (var. *nubigena*). I have lately received this variety (which is considered a rarity in England) from the Aran Isles, in Galway Bay, but little above sea level, and also from the Faroës. This is a good indication of the similar darkening effects produced in mountain, western, and northern regions.[1]

Specimens of the common wheel-web spider, *Epeira diademata*, which I have received from the Aran Isles, also show the same darkening tendency. The large conspicuous white spots in form of a cross, so characteristic of typical examples of this spider, are reduced in these western individuals to small dots. A similar, but less marked, variation is to be noted in specimens from co. Donegal.

Observations on this spider have furnished me also with a few facts bearing on the subject discussed in the earlier part of this review. The ground colour of the abdomen is well-known to vary from light brown through various shades of brown and red to a nearly black hue. Among granite rocks I have always found the latter variety; as it crouches in its retreat in a crevice of the rock, its black and white markings harmonise admirably with the mica and white felspar of the granite; but among herbage, the red and brown varieties are the more common. Whether this correspondence

[1] On the other hand, the black carrion beetle, *Silpha atrata*, is represented in Ireland by the var. *subrotundata*, which is generally of a brown colour.

be due entirely to the action of natural selection, or to some direct influence of the environment, experiment may perhaps decide. There can be little doubt that, to spiders, such correspondence is of "aggressive" as well as of "protective" significance.

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GEO. H. CARPENTER.

VII.

Experimental Embryology.[1]

EXPERIMENTAL Embryology is the youngest department of biological science; for although the idea of artificially influencing the germ is very old, and although even Swammerdam is said to have experimented in producing monstrosities, all the important results are very recent. They are already so important, both in themselves and in their suggestiveness, that it is imperative on every biologist to take stock of them. In part this has been already done by Weismann in his recent book on "The Germ Plasm," and by Roux, in an address delivered to the congress of the *Anatomische Gesellschaft*, held last June in Vienna; but already there are further researches of moment to be taken account of, and a fresh survey may be permitted. A critical review I do not propose to attempt, partly because that may be more profitably left to the expert embryologists, partly because it may be conveniently deferred until the body of facts has had time to become integrated.

I. Just as pathology sheds light on physiology—for instance, in the case of the thyroid gland—so teratology may help us to understand normal development. The most successful worker along this line has been M. Camille Dareste. He is the acknowledged chief of artificial teratologists. To attempt an appreciation of his results would lead us far into morphological questions; suffice it to say that he has experimented with the eggs of birds, placing them vertically instead of horizontally, hermetically varnishing part of the shell, keeping them slightly above or slightly below the normal temperature of incubation, heating different parts of the egg unequally, and so on. He has not only shown that the germ is plastic in the grip of its environment, he has been able to induce various malformations which are of interest to the student of morphology.

Dr. Bertram C. A. Windle, Professor of Anatomy in the Queen's College, Birmingham, has followed up some of Dareste's investigations, observing on the eggs of the fowl the effect of (*a*) excluding part of the air supply by varnish, (*b*) incubating under electrical current, and (*c*) incubating in the proximity of magnets. He is very cautious in his conclusions. He confirms, however, the view of Dareste that the same anomalies may be produced by diverse

[1] A Paper read before the Scottish Microscopical Society, March 10, 1893.

methods. "Whatever the disturbing agent may be which is employed, the failures of development are generally of the same nature —a consideration which, if true, as I believe, points to a much deeper explanation of the variations and malformations produced than that which attributes them to the actual deprivation of air, or whatever the means may be which is used." "I think there is now good evidence to prove that these disturbing agents act, at least in the great majority of cases, on that part of the developing organisation which is concerned with the formation of the vascular system of the embryo."

Professor O. Hertwig has recently (1892) published an elaborate essay, entitled "Urmund und Spina bifida," based on a study of abnormalities which occur in the development of frog ova in consequence of over-ripeness and polyspermy. The segmentation may be strangely irregular, large parts of the yolk may remain undivided, and the formation of the gastrula may be so disturbed that the blastopore or the primitive groove remains partially or wholly unclosed. He believes that the malformations which occur in the frog embryos in consequence of the imperfect closure of the "Urmund" are analogous to the Terata mesodidyma and katadidyma in Teleostean fishes and to Spina bifida in higher vertebrates. In the over-fertilised egg opposing factors conflict; on the one hand, there are forces tending to development and incited by the fertilisation, on the other hand, there are forces of an inhibiting and disturbing nature, due to the injury the ovum has sustained by over-maturation, or by other abnormal conditions operative before fertilisation.

II. We shall now notice some results of the important work of Dr. W. Roux. It is well-known that, in a number of types, the first three planes of segmentation in the dividing ovum correspond to the three chief planes dividing the bilaterally symmetrical adult into right and left, rostral and caudal, dorsal and ventral regions. In Amphibians and Ascidians, the first plane of segmentation in the ovum normally corresponds to the median plane of the embryo. This was shown for Amphibians by Newport, Pflüger, and Roux, and for Ascidians by E. van Beneden and Ch. Julin. Of the first two cells into which the egg of a frog develops, one has in it the material for forming the right half of the body, the other has in it the material for forming the left half of the body. It is a separate question whether each cell has, by itself, the power of forming half a body.

For a time, there was considerable difference of opinion as to the possible influence of gravity in determining the first segmentation planes. Pflüger (1883) maintained that gravity determined the arrangement of the molecules, determining, for instance, in the unsegmented ovum the position of the future nervous system or dorsal line; but Roux showed that when frog ova developed on a slowly rotating vertical wheel, so that the direction of the force of gravity was continually being changed, the segmentation remained normal. In

other words, the orientation of the ovum is regulated by internal conditions. Born reached the same conclusion by another method.

Having settled this point, Roux attacked the question whether all parts of the ovum were necessary to normal development. After two or more divisions, certain cells of the segmenting egg were pricked by a needle, so that some of the substance was lost. Thereafter, the development was watched until the embryo became differentiated. The result was that a loss of even one-sixth of the substance of the ovum produced only circumscribed local defects or disturbances. To develop fairly well, an ovum need not be intact.

Then Roux proceeded to exclude from development one of the two first segmentation cells. This was done by puncturing one cell with a hot needle. The result was that a typical half-morula, or half-gastrula, or half-embryo developed. Thus, there might be half cerebral vesicles, one auditory vesicle, a half-gut, a single row of protovertebræ, and a notochord of half the normal thickness. Sometimes, however, these half-embryos were slightly abnormal. Thus it was proved that one of the first two segmentation cells may form half an embryo; that it has not only the requisite vital material, but the requisite power; and, that it can develop apart from its neighbour. It is a likely inference that in normal development, a similar independence does to a certain degree obtain.

What is true when the first segmentation-plane corresponds to the future median one, is also true when the first plane corresponds to the future transversal, as is sometimes the case in the segmentation of frog ova. In other words, Roux was able to produce not only a right and left half-embryo, but also an anterior and a posterior half-embryo. By destroying one of the first four segmentation-cells, he was able to rear three-quarter embryos; by destroying three of the four, he got quarter-gastrulæ. Indeed, he goes the length of maintaining that the gastrulation of the frog ovum is normally a kind of mosaic work, formed in at least four vertical, independently-developing pieces.

Very remarkable is the process by which the half-embryo may form a whole by vitalising the injured half-egg which has been lying passive while the uninjured half has been developing. To this process Roux applies the term post-generation. Nuclei and, perhaps, also portions of protoplasm migrate from the uninjured side into the passive unsegmented mass; a remarkable kind of cell-division is set up; and gradually, in a peculiar manner which Roux describes, the missing half is post-generated. Quite lately, however, Roux has been able to rear an entire frog embryo from half an egg without any co-operation on the part of the other injured half.

III. Professor C. Chun observed in 1877 that when the two first segmentation-cells of a Ctenophore ovum were shaken apart, each formed a half-larva, with four ciliated ridges, four meridional vessels, and one tentacle. The half-larvæ actually became sexual, and by a process of budding, the half awanting was formed. This observation,

which was only published (through Roux) towards the end of last year, is the more interesting since Ctenophora are far removed from Amphibia. In both cases there is unequal segmentation of the ovum, and in both cases the formation of the missing half is long delayed.

IV. L. Chabry, experimenting with the ova of an Ascidian (*Ascidia aspersa*), obtained results which are, on the whole, similar to those of Roux. His method is to place the ovum in a glass tube, of almost equal diameter, and to puncture one of the first two segmentation-cells, observing it, meanwhile, under the microscope. The cell dies; the survivor forms a typical half-morula, half-gastrula, a right or a left half-larva. If the two anterior cells of the four-celled stage be destroyed, a posterior half individual results. Quarter and three-quarter forms were also obtained. Moreover, Chabry believes that he has been able to detect the cells whose descendants give rise to eye, notochord, attaching papillæ, &c. On to the sixteen-cell stage, at least, each cell has a determined destiny, it represents a definite part of the embryo; if a cell be destroyed, the defect in the larva is a definite one. It may be noted that Roux does not think Chabry's figures of half-gastrulæ really justify the title, it seems to him as if a regenerating process had already begun to operate so as to complete the embryo. There is not, however, any revitalising of the injured half-egg, for the injuries are fatal.

V. Carl Fiedler (1891) experimented with the ova of Echinoderms (especially of *Echinus microtuberculatus*), first trying the puncturing method, afterwards following the Hertwigs' method of shaking the eggs. When one of the first two segmentation-cells was punctured, so that it merely lost some of its substance, it did not die: it recovered and divided as usual, except that the cells to which it gave rise were smaller than those of the other side. The blastula was unsymmetrical, the embryo normal. But when one of the first two segmentation-cells was punctured so that the nucleus was fatally injured, the cell died; the surviving cell formed a half-morula, a half-blastula, and, perhaps, even a half-gastrula. When two cells of the four-cell stage were injured, a half-development also resulted, and that the same whatever pair of cells remained. Therefore, the first four cells are equivalent; but at the eight-celled stage this equivalence is lost, for different groups of four turn out differently.

VI. Hans Driesch (1891) also experimented with the ova of sea-urchins, pursuing the shaking method. When the first two segmentation-cells were shaken apart, one of them usually died, the survivor divided into a half-morula, as was the case in Fiedler's experiments; but the half-morula formed a closed blastula of less than the normal size. Then followed a small gastrula stage, and a minute Pluteus larva. Thus, from one of the first two segmentation-cells a normal but minute larva may develop. While Roux got half-embryos from half-an-egg, Driesch got half-sized but otherwise complete embryos.

Driesch is strongly opposed to the conception first clearly stated by His, that there exist in the germ organ-forming regions, for the marginal material of a left half-morula may become part of the median region, and eventually part of the right flank of the Pluteus. Moreover, in some cases, the first two segmentation-cells shaken apart give rise to distinct twins, or if the separation be imperfect, to double organisms, at least, to double blastulæ. In one case a double Pluteus was obtained from the imperfect separation of the halves of a blastula.

In a second paper, Driesch gives the results of further experiments on the eggs of sea-urchins (*Sphærechinus granularis* and *Echinus microtuberculatus*). Vejdovsky, in his account of the development of *Allolobophora trapezoides*—an earthworm whose eggs very frequently form twins—had suggested that the twinning was, perhaps, influenced by warmth, for it was most frequent in warm weather. This suggestion (for speculative suggestions are often valuable) prompted Driesch to try the effect of increased warmth on the developing ova of sea-urchins. The result was very striking. Almost all the eggs of *Sphærechinus* formed distinct twin blastulæ, gastrulæ, and even Plutei. In one case a connected twin gastrula was observed. The eggs of *Echinus* did not respond to the warming; and sometimes, strange to say, all the warmed eggs of *Sphærechinus* turned out single normal embryos. Still, that there is a relation between warmth and twinning seems likely.

Many experiments were made with the four-celled stage. If one cell was shaken off, or pricked to death, the three-quarters left almost always developed quite normally into Plutei. Yet the details of segmentation were of course different. To remove one of the first four cells does not hinder development to any appreciable extent.

But, more than that, an isolated cell of the four-celled stage, isolated by bursting the other three, develops as if it were still in its natural alliance. Very few of these, however, got beyond the blastula-stage, but two became Plutei. A quarter, a half, or three-quarters of the four-celled stage may therefore form a fully developed larva.

Selenka, in his studies on the development of marine Planarians, had observed that increased temperature produced deviations from the normal segmentation. This led Driesch to experiment. He found that with increased temperature the formation of smaller cells or micromeres is wholly or partially inhibited, and that in other ways the segmentation may be disturbed, but the striking fact is that slight changes in the position of the segmentation-cells, and even a modified type of segmentation, do not hinder the development of a normal organism.

Driesch proceeded to experiment on the effect of pressure, which was supplied by the weight of a cover-glass resting in part upon the ova. The nuclear spindles were disposed at right angles to the direction

of the pressure. The formation of micromeres was suppressed; the sixteen-cell stage was a plate; and yet typical Plutei were formed when the pressure was removed. In other cases, a two-layered plate of sixteen cells was formed, and yet normal Plutei resulted, a fact which seemed to Driesch definitely to contradict the conception of His that there are definite germinal areas. What should have formed one pole of the embryo formed the two sides, and what should have formed the other pole of the embryo formed both poles. In fact, the segmentation-cells of sea-urchins are on to the sixteen-cell stage markedly homogeneous, for even after a very abnormal development normal larvæ may result.

The Hertwigs have shown that when doubly-fertilised the ova of sea-urchins divide simultaneously into four. In such ova, which Driesch believed to have been doubly-fertilised, the whole rhythm of cell-division was double, so that the sixteen-cell stage was not a true sixteen-cell stage (with four micromeres) but a double eight-cell stage. But none of these forms developed.

VII. MM. Pouchet and Chabry, experimenting in 1889 with the developing eggs of sea-urchins, found that when some of the lime-salts in the sea-water were replaced by potassium or sodium oxalate, the skeleton of the larva was incomplete or entirely absent. An apology for a Pluteus with food-canal and other organs, but without skeleton, was formed, even when nine-tenths of the lime was got rid of. Further reduction of the lime inhibited all development, even gastrulation.

VIII. Following the researches of Pouchet and Chabry, the Hertwigs, and others, Herr Curt Herbst of Zürich has recently made an interesting series of experiments on the ova of sea-urchins. To the sea-water in which these were developing he added solutions of various salts, usually in the proportions of 3·8 grms. to 100 cm. of sea-water. Each experiment was checked by a control experiment in which the conditions were the same, excepting the addition of the salt-solutions. Fertilised ova were always used, to avoid the complications of polyspermy. The ova were those of *Sphærechinus granularis*, *Echinus microtuberculatus*, and *Strongylocentrotus lividus*, and in the three cases the results were practically the same.

Forty-four experiments were made in which so much of the sea-water, *e.g.*, 140 c.cm. out of 2,000 c.cm., was replaced by a 3·7 per cent. solution of KCl in ordinary water, which contained a considerable quantity of lime. So the observer had to deal with the addition of something new rather than with the removal of much lime.

The formation of the calcareous needles was delayed; when they appeared they were abnormal and always incomplete. The internal structure of the Pluteus was distinct, but the characteristic processes were small and rounded off. If there was less than 7 per cent. of the KCl solution the abnormality of the larvæ was less pronounced.

These results are not peculiar to KCl, for experiments with

KBr, KI, KNO_3, K_2SO_4, NaI, $NaNO_3$, $MgSO_4$ gave somewhat similar results. It is therefore possible by adding these salts to the sea-water to disturb the metabolic processes by which the calcareous skeleton is formed and the growth by which the peculiar Pluteus processes are formed, while the fore-, mid-, and hind-gut, the vaso-peritoneal vesicles, and the ring of cilia, develop uninfluenced.

The inhibition of the characteristic Pluteus outgrowths is interpreted by Herr Herbst as due to absence of the requisite stimulus naturally provided by the calcareous rods; for he believes, it seems to me with much likelihood, that the rods afford a stimulus to growth in those parts where they occur.

When the solutions of the added salts were made in distilled water, with the obvious result of reducing the amount of lime in the aquarium, no needles of lime were formed even by the seventeenth day, although the cells which ought to have contributed to form them were then seen arranged in their typical disposition.

Another remarkable result was the production of larvæ (gastrulæ) in which the tuft of long, stiff processes, which normally occurs at the so-called animal pole, and is probably directive, was hypertrophied into a protruding knob with small mobile cilia. This observation was made at Trieste in 1891, but it did not succeed at Naples in 1892, nor again at Trieste in 1892, a divergence which the observer accounts for by the different temperature-conditions which obtained.

But, perhaps, the most remarkable results which Herr Herbst was able to make sure of, were obtained by adding salts of Lithium to the sea-water. The normal blastula elongates, constricts into two portions—one thin-walled, the other thick-walled. They differ also in pigmentation and ciliation. Between these two primary parts a connecting region is formed. In the majority no calcareous needles appeared. Very frequently the thick-walled portion grows at the expense of the thin-walled portion, which dwindles to a mere cap. In rare cases a gut and a mouth appeared. Now, had these larvæ lived a little longer, it seems quite likely that they would have reached the Pluteus stage to which they were approximating. If so, they would have reached it by an entirely abnormal path. But, without making any suppositions, the results are remarkable enough. It seems that the thick-walled portion is nothing more nor less than the protruded archenteron—a sort of embryonic hernia; that the thin-walled portion is the outer wall of the gastrula, and that the connecting portion is the hind-gut of the Pluteus.

It is interesting to observe that the potency of the Lithium salts decreases from the Chloride to the Iodide; in fact the potency is in inverse ratio to the molecular weight. So is it also with salts of Sodium and Potassium. The rule only applies to salts of one and the same metal.

It seems to have been this fact, taken along with researches of Hofmeister, Heidenhain, and others, that led Herr Herbst to the

conclusion, into the demonstration of which we shall not enter, that the influence on the developing ova was not directly chemical, but was due to the altered osmotic pressure of the sea-water. The changed chemical composition produces a changed osmotic pressure, this acts as a stimulus on the cells of the larva in such a way that they are diverted from their ordinary course of development.

IX. Mr. Edmund B. Wilson, of Columbia College, New York, who is well known for some valuable contributions to the embryology of Invertebrates, has recently published a preliminary account of experiments on the developing eggs of Amphioxus.

It is well known that the first stages in the development of Amphioxus are simple. The fertilised egg divides into two, into four, into eight cells, and so on until a blastula is formed whose cells are slightly larger in the lower hemisphere. By invagination the blastula becomes a gastrula.

By shaking the water in which the two-celled stages floated, Mr. Wilson produced two quite separate and independent twins of half the normal size. Each of the isolated cells segments like a normal ovum, and gives origin, through blastula and gastrula stages, to a half-sized metameric larva.

If the shaking has separated the two first segmentation-cells incompletely, double embryos—like Siamese twins—result, and also form short-lived (twenty-four hours) segmented larvæ.

Similar experiments with the four-celled stages succeeded, though development never continued long after the first appearance of metamerism. Complete isolation of the four cells resulted in four dwarf blastulæ, gastrulæ, and oval larvæ. Separation into two pairs of cells resulted in two half-sized embryos. Incomplete separation resulted in one of three types—(*a*) double embryos, (*b*) triple embryos —one twice the size of the other two—and (*c*) quadruple embryos, each a quarter size.

The eager observer proceeded to shake up the eight-celled stages, but in no case did he succeed in rearing a gastrula from an isolated unit of the eight-celled stages. Flat plates, curved plates, even one-eighth-size blastulæ were formed, but none seemed capable of full development.

This is most interesting. A unit from the four-cell stage may form an embryo, a unit from the eight-cell stage may not. "The inability to produce a complete embryo may be due either to quantitative or to qualitative limitations." Perhaps the one-eighth-cell has too little living stuff; perhaps it is already too much differentiated to regenerate the whole. We know that there is a size limit to the fragments of Hydra which will regrow an entire organism.

According to Mr. Wilson, two facts tell against supposing that the limit is quantitative. In the first place the one-eighth products swim actively, and live as long as the quarter embryos. In the second place, minute gastrulæ may be produced from two- or four-celled

stages, which are even smaller than one-eighth the normal size. These probably result either from fission of the half or quarter blastomeres, or by some fragmentation due to the shaking-up. "These minute embryos prove that a mass one-eighth the size of the normal ovum, or less, is capable of producing a gastrula," yet the one-eighth blastomeres did not.

It seems to me, however, that this is not quite conclusive; for although I am not one of those who exalt the nucleus at the expense of the cell-substance, it seems that we cannot exclude the supposition of a quantitative limit until we know more about the size and state of the nucleus (*a*) in the fraction or fragment of the two- or four-celled stage which did form a gastrula, and (*b*) in the one-eighth blastomere which did not form a gastrula. Yet, on the whole, I should agree with Mr. Wilson that the limitation is most likely to be qualitative, for by the eight-celled stage the difference between micromeres and macromeres has become pronounced. In short, the embryonic cells are beginning to be specialised.

It is very important to notice that the segmentation of the isolated blastomere of the two- or four-celled stage agrees exactly with that of the entire ovum. "The isolated blastomere develops as a unit, not as a half-unit, and the two cells to which it gives rise cannot be individually identified with those of a normal embryo-half. The development is transformed from the beginning; but in sea-urchin (Driesch) and frog (Roux) the development is at first that of an embryo-half, which subsequently, much earlier in *Echinus* than in *Rana*, gives rise to a perfect embryo by regeneration."

X. It is well-known that the entrance of a spermatozoon into an Echinoderm ovum is at once followed by the appearance of a thin enveloping membrane, which rises from the surface of the ovum and prevents the entrance of other spermatozoa. The spermatozoon which has entered influences the ovum so that others are excluded. It is not, however, certain that this membrane is really necessary for the prevention of polyspermy.

Now, the Hertwigs showed in 1887 that if unfertilised ova were placed in sea-water shaken up with chloroform, a protective membrane was formed. Herr Herbst has recently repeated the experiment, shaking up 1 c.cm. of chloroform with 50 c.cm. of sea-water. All the unfertilised ova placed in this mixture formed the protecting pellicle. He also succeeded when he used, instead of chloroform, benzol, toluol, xylol, creosote, or clove-oil, but the two last produced a pathological appearance in the cell-substance of the ova.

The protective membrane is really referable to the hyaline marginal layer of the ovum, which seems to have a greater consistence than the internal cell-substance; it has its analogue around the larval stages; in short, it is not in itself in any way remarkable. What is remarkable is the manner in which this marginal layer is deliminated and hardened after the entrance of a spermatozoon. The hardening must

be due to an influence exerted on the cell-substance by the entrant spermatozoon. The delimitation is due, according to Fol, to the coagulation of a gelatinous substance between the envelope and the ovum. The Hertwigs and Herbst have shown that the hardening and the delimitation may be artificially evoked.

Furthermore, Herr Herbst shook off the membrane from fertilised ova, and found that in a benzol mixture a second membrane was formed. He even succeeded occasionally in producing a second membrane in addition to the first in fertilised ova, and in producing two concentric membranes around unfertilised ova.

The net result of this interesting little research is to show that the conditions productive of the hardening and delimitation of the cortical layer of the ovum are to be found in the ovum itself, and are normally due to a stimulus associated with the entrance of the spermatozoon, but which may be artificially replaced by the stimulus of certain chemicals.

Yet Weismann asks us to believe that the spermatozoon has no dynamic or other than merely quantitative influence on the ovum.

There are many other recent researches which should be considered in a full discussion of experimental embryology, but I shall refer only to two more.

XI. Very striking is Boveri's experiment, on which Weismann lays great emphasis, and which one would like to see confirmed. Boveri artificially removed the nucleus from the ova of a species (A) of sea-urchin, and poured over them the spermatozoa of a related species (B). The ova, without any maternal nuclei, seemed to have their deficiencies supplied by the spermatozoa, and the larvæ which resulted had the characters of species B.

To Weismann, this case supplies direct proof of the all-importance of the nucleus in transmission. I must confess to some doubt as to the certainty with which specific characters of sea-urchins can be discriminated in the larvæ. Moreover, hybridisation has sometimes strangely one-sided results, and from the non-appearance of any maternal characters, one cannot conclude with certainty that the cell-substance is only of nutrient importance in development. Furthermore, until definite proof to the contrary is forthcoming, it seems well to continue to believe that fertilisation is due to the spermatozoon and not merely to its chromatosomes.

XII. Finally, we may refer to Heape's remarkable experiment, in which, from an Angora doe rabbit (fertilised 32 hours previously by an Angora buck) he transferred two ova into the upper end of the Fallopian tube of a Belgian doe rabbit (fertilised three hours before by a Belgian buck). When the Belgian doe gave birth, four young were Belgian, two Angoras.

A speculative discussion of some of the researches cited above will be found in Weismann's "Germ Plasm" (pp. 134–144), and to this the reader may in the meantime be referred. I prefer in this summary to leave the facts to speak for themselves.

In regard to the scientific utility of these researches in experimental embryology, it may be noted that a number of general results are very clear. (1.) There is a great deal of life in an egg. Three-quarters, a half, a quarter will in favourable conditions form a complete larva. (2.) There is no little plasticity in the germ; the segmentation may be profoundly altered, the shape of the young embryo may be greatly changed, and a new type of larva may be produced. Yet the inherited characteristics are strong, for the experiments show a marked tendency in the germ to reach a normal result even by an abnormal path. (3.) To analyse out a definite factor—say osmotic pressure—in teratogeny is beyond dispute useful, for it may help us not only to understand malformations occurring among higher vertebrates, but also to get nearer an understanding of the conditions of normal development. (4.) As everyone knows, there are few facts, or, some would say, no facts, which can at present be cited with confidence as direct evidence of the transmission of environmentally-produced variations; and Weismann's case against the possibility of acquired somatic variations specifically affecting the reproductive cells is strong enough to lead many to depreciate, except in the case of simple Protozoa and Protophyta, the direct influence of the environment as a factor in the origin of transmissible variations. Be that as it may. How many ova are there which float in the sea and in other media; these are now, as similar ova have been in the past, exposed to the influence of very complex physical and chemical conditions; that their living stuff may be greatly affected the results of experimental embryology show; it is likely that the same is true in Nature's great laboratory; and the results, being germinal, may be transmissible. We need not be in haste to exclude the direct influence of the environment from among the primary factors of evolution.

One practical application occurs to me. Many abnormalities in the segmentation of the ova of littoral animals, *e.g.*, Gasteropods, have been noticed by various zoologists. The other day, in examining the ribbons of eggs which Doris lays in such abundance on the low-tide rocks, I noticed the great frequency of twins and triplets. In some cases they seemed to preponderate. Is it not likely that an explanation may be found in the fact that the ribbons are battered to and fro by the surge? What is done in the laboratory may also occur on the shore. The shaking may separate the segmentation-cells and mechanically produce twins. This is, of course, nothing more than a suggestion, in default of the obviously necessary experimental verification, but I find an interesting confirmation in C. Chun's observation that twin Ctenophora were most abundant after stormy days.

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J. Arthur Thomson.

SOME NEW BOOKS.

Éléments de Paléontologie. By Félix Bernard. Première Partie. 8vo. Pp. 528, with 266 figures. Paris: Baillière & Sons, 1893. Price 10 francs.

The position of Palæontology among the sciences is somewhat doubtful, and even a palæontologist may question its right to have a text-book at all. Do we not rather need books of the following kind?—first, a systematic zoology, embracing all creatures whether living or extinct, and arranged, according to the best modern lights, on a phylogenetic basis—a work that should illuminate the obscurities of the present by the light of the past, and show the true meaning of perished organisms by reference to their living descendants; secondly, a good stratigraphical geology, correlating, as nearly as possible, the beds of different countries, without laying undue stress on those of the author's native land, then tracing through these beds the histories of the various faunas all over the world, and, from the combined evidence, sketching out the evolution of land and sea masses from the earliest times to our own day. To such ends, at least, our endeavours are directed, and to the ultimate perfection of both these as yet unwritten books every palæontologist makes his contribution. The worth of that contribution is proportional to the amount of zoological and geological knowledge possessed by the contributor. It is idle to select from the garner of the zoologist just those facts relating to the hard parts of animals, and from the geologist's note-book such information as bears only on locality and horizon. What is a palæontologist without a knowledge of embryology, of geographical distribution, of marine life, sedimentation and currents, of some mineralogy and petrology, and of changes induced by metamorphism? And yet, even if it were possible, it would surely be absurd to collect all these diverse bits of information, however valuable, and, putting them between two bits of pasteboard, to call the whole a Text-book of Palæontology.

Nevertheless, the present dearth of competent British palæontologists—as evidenced by the failure of the Indian Survey to find other than Germans—when regarded in connection with the abundance of pure zoologists and geologists, seems to show that palæontology, in practice, if not in theory, must have its special study. Let us, then, concede that it may be convenient for the student to have his special text-book, and let us consider what it should contain. The contents may be arranged under three headings:—(i.) The general principles of the science; in other words, the principles of biology and of geology so far as they affect the history of extinct animals. (ii.) Practical aids to research, including an account of the way to collect, clean, dissect, and investigate fossils, a guide to bibliography, and hints on the working out of problems and the proper way of publishing results. (iii.) The main facts in the history of extinct animals, which should be related as to one already acquainted

with geology and with the structure of living animals. Of course the above remarks apply with equal force to plants and the students of them.

These thoughts have been suggested by a Text-book of Palæontology from the pen of Dr. F. Bernard, of which the first part has just been published. We may now examine how far it fulfils our demands.

Of the three parts to be comprised in our ideal text-book, the second is here absent, if one excepts an occasional bibliographic reference and two or three pages on fossilisation. The first part, or "Généralités," though unduly compressed into 76 pages, is exceptionally good. It is well abreast of the times, and gives a fair and well-balanced account of the many widely-differing "doxies" that constitute modern palæontology. Thus, a chapter on Palæontology and Evolution contains excellent remarks on the conception of the species and how it has been modified by palæontological investigations, on the character, methods, and causes of variation, on adaptation, correlation and parallelism, and on almost every biological theory to which a name has of late years been given. Other chapters deal with phylogeny, with the distribution of organisms in past ages according to the conditions of environment, with methods of fossilisation, and with the main characters of the geological periods. In various places the author alludes to the minute research required of modern palæontologists, the investigation of every detail of structure in the face of difficulties that appal the zoologist, the comparison of all stages of growth and of every conceivable variation, the analysis of enormous masses of material, and the tracking of lineages from one horizon to another; and he rightly points out how all this can only be accomplished by the examination of innumerable well-authenticated specimens, and how progress must be continually aided by the increase of our collections.

The remainder of the volume is devoted to Animal Palæontology, which, for the present, only reaches the middle of the Mollusca. This part is by no means so satisfactory; the plan, indeed, is good, but the execution is faulty. In each group, after general remarks and a description of the morphology, the main forms are systematically alluded to, after which their distribution and relationships are treated of. This is right enough, but it is hard to see how much the reader is expected to know already. A person who accepts the term "zoophytes," which our author applies to sponges and echinoderms, would surely be puzzled by the word "polyp" suddenly introduced without explanation under the heading Hydromedusæ. In further support of our condemnation of this part it will only be necessary to quote a few sentences. Thus, the Echinoderms are not only called zoophytes, but their body is said to be "entirely covered with calcified dermal plates." How about most Holothurians? "Les interradiales n'existent pas chez les Crinoides actuels [*Thaumatocrinus*!]: elles se présentent toujours chez les fossiles [*Encrinus*!]." Pinnules are defined as "petits appendices creux"; when they are absent, as in *Cyathocrinus*, covering-plates are said to be present, and then the ventral groove is separated from the canal that contains the axial cord. It is hard to believe that a man who could write such incomprehensible nonsense is a pupil of a leading authority on Echinoderms—Professor Perrier. We will spare the author and our readers from further quotation. The excuse is ready enough. It is impossible nowadays for one man to write

an adequate systematic zoology, even though he confine himself to animals no longer living; the field is far too vast, the literature cannot be coped with, and the pity of it all is that young men fresh from the examination-room can only be taught their own presumption by a fatal experience.

Despite its inevitable faults, we recommend this book to students for the value of its introduction, and for its numerous good illustrations; while, to more advanced workers, it may prove useful as setting forth the views of the French school on many subjects, some here published for the first time. We will only warn the unsuspecting against the specific names at the foot of many of the figures; the misprints, which this book shares with others issued by the same publishers, they will discover for themselves.

F. A. Bather.

Fauna und Flora des Golfes von Neapel: xix. Monographie; Pelagische Copepoden. By Dr. W. Giesbrecht. 4to. Pp. 532, pls. 54. Berlin: Friedländer and Sohn, 1892. Subscription price 50 marks.

The magnificent series of "Naples Monographs," of which the present is the latest, shows no symptoms of a falling off either in excellence of matter or in the beauty of illustration and printing. Nor do the volumes decrease in size; in fact, they seem to get bulkier with increasing age. Those of us who are interested in the work of the Marine Biological Association's Laboratory at Plymouth naturally feel some envy at the sight of the nineteen volumes which represent the industry of those who are officially connected with the Naples Station, or have enjoyed the great facilities for work there offered. It is greatly to be regretted that the list of subscribers to the funds of the Marine Biological Association is too small to permit of a sufficiently large expenditure of money upon publications. It is rather tiresome, too—the writer is naturally expressing his individual opinion only—that so much purely utilitarian work has to be done, to the partial, but, fortunately, not complete, exclusion of the more interesting lines of Biological enquiry, so admirably developed at Naples.

Dr. Giesbrecht's work is illustrated by fifty-four double plates, of which the first five are devoted to the representation of the living Crustacea with all their natural colours. These plates are so splendid that we feel ourselves unable to praise them adequately without indulging in language of too fulsome a character. The brilliancy of colour often developed in these small creatures is most marvellous; but while some, such as *Sapphirina ovato-lanceolata* (pl. i., fig. 7), exhibit literally all the colours of the rainbow, others are quite plain. We wait anxiously to hear authoritatively the reasons for this display of adornment. The variety of colour even extends to the eggs; they are blue, green, red, olive, or purple—in fact, they are only rivalled in variety by the eggs whose use is peculiar to that season of the year which has just passed. The remaining plates illustrate chiefly a collection of detached appendages of the various species dealt with. The development of these appendages is often quite extraordinary, while the long plumed hairs which they and the body-surface frequently produce are doubtless suggestive of the larval forms of the higher Crustacea. The volume is entirely systematic and faunistic in scope; the anatomy will be treated of in a second part.

Ordnance Survey. Report of the Departmental Committee appointed by the Board of Agriculture to inquire into the present condition of the Ordnance Survey. London: [Parl. Paper C.—6895], 1893. Price 4½d.

There are, we suppose, few readers of this journal who do not take with them upon their perambulations sheets of the one-inch Ordnance Survey maps of the district they are visiting. As, however, there may be some such people, and as it a golden rule that one cannot too often be reminded of a good thing, we call attention to this Blue Book just issued. The report opens with a history of the Ordnance Survey, which may be said to have begun with the measurement of a base-line on Hounslow Heath in 1784. The one-inch to a mile map was commenced in 1797, and continued steadily till 1824, when the whole of the South of England and part of Wales were completed. In 1824 an Irish survey was commenced on a scale of six inches to a mile, and this was extended later to the whole of Scotland and the six northern counties of England; the one-inch maps issued since then having been reduced from those on the larger scale. Since 1855, moreover, the whole of the country has been surveyed anew on a different system, and the result of this work is seen in the "new series one-inch map," so well-known and so valuable to everyone.

After the "history" comes the "methods and processes of the Ordnance Survey," which need not detain us here. The Committee was appointed to consider (1) What steps should be taken to expedite the completion and publication of the new or revised one-inch map (with or without hill-shading) of the British Isles? (2) What permanent arrangements should be made for the continuous revision and speedy publication of the maps (1:500 (towns), 25-inches, 6-inches, and 1-inch scales)? (3) Whether the maps, as at present issued, satisfy the reasonable requirements of the public in regard to style of execution, form, information conveyed, and price; and whether any improvements can be made in the catalogue and indexes? To the first of these questions the answer given is that the continuance of the Temporary Edition ("Advanced Edition") of the maps by photo-zincography seems desirable.

To the second question, the Committee replies: "The 1-inch map is the one most used by the public for general purposes, both military and civil. We recommend strongly that its revision be carried out at all times independently of that of the larger scales, and that whatever funds are necessary should be provided to carry out these recommendations at as early a date as possible." That the "Cadastral Maps" (5 feet, 10 feet, 25 inches, and 6 inches to the mile), in consequence of the largeness of the scales, rapidly get out of date. "Scarcely any of them have been revised [since 1854], and it is urgently necessary that the advantage of so splendid a work as these maps of Great Britain and Ireland undoubtedly are should not be destroyed for want of a regular system of revision." With regard to the "town maps," they very properly advise that the municipal authorities "should be placed under a statuteable obligation to maintain and correct a copy of that map, showing all alterations in the town inserted in accordance with instructions and regulations to be issued by the Ordnance Survey Department . . . and that failing such proper maintenance, the ordnance surveyors should do what is found to be necessary, and charge the cost to the town." At present this revision is done in a few towns (*e.g.*, Edinburgh) by private publishers, who keep the maps up to date, and actually sell their own correct version to the displacement of the Government Survey.

Considering the enormous number of sheets these "town maps" occupy (13,860), and the small sale, the Committee seems to incline to the view originally put forward by Sir Charles Trevelyan in 1854, that the engraving of "town maps" is unnecessary, and that MS. copies might be supplied at a less cost than that entailed by the engraving and storing of so many plans.

With regard to the third question, the Committee deals with so many criticisms, complaints, and suggestions that it is impossible to quote them here; but they are all fully dealt with in the Report. The most sensible of all, perhaps, is the suggestion "That an edition of the 1-inch map, and of maps on a larger scale on *thin tough* paper be issued," and this the Committee says (p. xxx.) "might probably be serviceable for the 1-inch map." Another criticism, the faulty spelling of names, has been a long-standing grievance, but, as the Committee remarks, a most difficult one to cope with. They give some examples, but hardly any, perhaps, as curious as that of "Silex Bay," near Flamborough Head, the effort, possibly, of some astute surveyor with a smattering of geology, when glancing at the chalk cliff, to take down the local pronunciation of Selwick.

The Committee most truly says, "No country at the present time possesses anything as perfect and complete as our cadastral survey, published by the authority of the Government, and available at a moderate price to every person in the kingdom, showing every plot of ground and every isolated building, and having the new 1-inch map founded on it by the accurate copying power of the photographic lens." Whatever faults exist, this sentence is in the main uncontrovertible, and we sincerely hope that due provision will be made for the continuance and revision of our Ordnance Survey in as liberal a manner as the splendid results already achieved deserve.

A Manual of Bacteriology. By A. B. Griffiths, Ph.D., F.R.S.E., F.C.S. London: William Heinemann, 1893. Price 7s. 6d.

There is a suggestion of book-making about this volume. The frontispiece is a view of the outside of the Pasteur Institute; there is a ground plan and a view of the interior of the Edinburgh Bacteriological laboratory; a representation of a Zeiss' microscope; of microphotographic apparatus; microtomes; needles and knives, and much other delectable matter. The drawings of microbes themselves are not numerous or particularly clear. The suggestion of collation is by no means dispelled on reading. A fair account of microbes and the various methods of investigating them is given, but there seems no special reason why Dr. Griffiths rather than anyone else should have written the book. No doubt it is intended to be more elementary, but we miss the breadth of view and clearness of exposition to be found in Dr. Sims Woodhead's volume on "Bacteria and their Products." The publisher is, however, to be congratulated on the excellent get-up of the book.

Birds of Germany. "Deutschland's nützliche und schädliche Vogel." By Dr. Hermann Fürst. 8vo text, and fol. coloured pls. Pt. i. Berlin: P. Parey, 1893.

As a perfect marvel of cheapness, combined with the highest style of artistic excellence, this work, of which the first part is before us, can have but few if any equals; and as most of the birds of Germany are

common to our own country, it ought to command a large sale here, as well as abroad. The special object of the work is to enable all such as are engaged in rural pursuits to recognise the ordinary birds with which they may meet, and, at the same time, to distinguish between those which are harmful and those which are useful either to the agriculturist, the gardener, or the gamekeeper. With this object, it has been considered of prime importance that the illustrations should be on such a scale as to render the task of identifying a bird as easy as possible without the trouble of wading through descriptions. Accordingly, it is announced that the smaller and medium-sized species are to be figured of the natural size, while the larger kinds are to be reduced by one-half. Each part is to comprise four double folio plates, lithographed in colours, and accompanied by a sheet of 8vo text; the price of each part being three shillings (marks). The work is to be completed in eight parts; and if the execution of the plates is kept up to the standard of those in the first part, it will place trustworthy and artistic likenesses of a large number of European birds within reach of all, at an exceedingly low price. R. L.

L'Aquarium d'eau douce. By H. Coupin. (Bibliothèque des connaisances utiles.) 16mo. Pp. 348, 228 figures. Paris: Ballière et Fils, 1893. Price 4 fr.

This useful addition to Ballière's Library is written for young naturalists and those who take a casual interest in natural objects. The author treats of the aquarium and its forms, of water and its aëration, of the proper plants and their effect on the purity of the water, of methods of capture, and of the various groups of animals, giving a sketch of the life-history and other matters interesting for observation. The book is copiously illustrated, and although the figures are in some instances rather crude, they will be of considerable use to those for whom the book is intended. We are glad to see the footnote on p. 91 regarding the reversible Hydra.

Catalogue of the British Echinoderms in the British Museum (Natural History). By F. Jeffrey Bell. London, 1892.

A catalogue is hardly expected to furnish interesting reading; one does not exactly look into the British Museum publications of this kind with any expectation of finding easily assimilable tit-bits of zoological information; and yet this rule, like many others, is not without its exceptions. Dr. George Johnston's contribution to the series is full of highly readable and instructive notes concerning the "non-parasitical worms." Mr. Bell's catalogue is written on the plan of some of the more recently-issued volumes; that is to say, it is moderately learned and very arid, but, unlike many of them, it is restricted to an account of the inhabitants of the shores of Great Britain and Ireland. "One of its objects," writes the author, "is to supply the student of the British Marine Fauna with a handbook" —to the Echinoderms of our shores. Dr. Günther hopes in the preface that the study of Echinoderms will be encouraged by a perusal of this volume. We think, however, that the public, to whom the catalogue is addressed, will come to the conclusion that the study of the Echinoderms is fraught with too much contentious matter, and that it is as thorny as are the objects with

which it deals; for one quarter of the author's preface is devoted to an attack upon Mr. Sladen's "Challenger" volume upon the Starfishes. We are not concerned with the issue of this dispute, though, incidentally, we may remark that the opinions of a gentleman who has made material additions to the morphology of the Echinoderms are not treated with the respect that they should have met with at the hands of the author of the catalogue before us. The bracketed remarks in the following phrase strike us as not being at all in place. Mr. Bell says that Mr. Sladen repeatedly expresses "views for which he does not give (I do not say, does not possess) adequate reasons." The bellicose tone which is adopted throughout is not suited to the nature of the publication; and we desire to protest against its use in an official catalogue.

As to the catalogue itself, the list of genera and species is preceded by some "Introductory Remarks," occupying ten pages, and consisting of a sketch of the anatomy of the group. This sketch can hardly serve any useful purpose. It cannot be pretended that it is an adequate account of the structure of the Echinoderms; a more detailed and illustrated description of the hard parts alone would have been useful, as they are principally used for classification purposes; the catalogue is written by a systematist for systematists, and we cannot see why he should have attempted anything more ambitious.

In fairness to the author, we ought to say that the actual catalogue itself seems carefully and conscientiously done; he is here in his natural element; and there is a freedom from misprints which indicates much laborious work on the part of the author and (we presume) the editor. We cannot be too thankful that persons exist who are capable of doing and willing to do this necessary but uninteresting work for us.

THE EARTH'S HISTORY. An introduction to Modern Geology. By R. D. Roberts, M.A., D.Sc. [University Extension Manuals.] Crown 8vo. Pp. 365, with 8 plates and 51 illustrations in the text. London: John Murray, 1893. Price 5s.

UNDER the above somewhat ambitious title, Dr. Roberts adds one more to the numerous geological text-books. The volume is nicely got up, and contains several useful maps, printed in colours; but beyond this we cannot praise the book, for it contains only the well-known materials worked up again. One does not, of course, expect much originality in a text-book, but the student has a right to ask that the author shall make a good selection, and arrange the matter systematically.

HANDBOOK OF THE IRIDEÆ. By J. G. Baker, F.R.S., F.L.S. 8vo. Pp. 247. London: George Bell & Sons, 1892. Price 5s.

WE gladly welcome another addition to the valuable series of monographs by Mr. J. G. Baker, of Kew. The present volume is the fourth of a series devoted to certain groups of plants that enter largely into horticulture. To many it will be the most interesting of the series, for it deals with such handsome and well-known genera as Iris, Crocus, Ixia, and Gladiolus, besides a host of less familiar forms, unknown except to the systematic botanist.

Few, even of the professional gardeners, will have any idea of the wealth and variety to be found in this group of plants. Iris, for instance, has no fewer than 161 species, Crocus has 66 species, Ixia 24, and Gladiolus as many as 132. Plants of this order have, usually, such large and showy flowers, and many of them cross so freely, that they are particularly adapted for experiments on cross-fertilisation. For this reason, as well as for their beauty, we should like to see the group more fully represented in our gardens and greenhouses. Mr. Baker's "Handbook," we may observe, is in English. The author gives, besides the technical descriptions and habitat, full references to the earlier authorities, and, in the case of rare forms, he mentions the original discoverer.

Those who work upon the Tertiary Fossils of the United States will be gratified to hear that Dr. W. H. Dall has successfully grappled with the difficult task of determining the dates of publication of Conrad's books—"The Fossils of the Tertiary Formation" and the "Medial Tertiary." These books were issued in separate parts, all of which are now exceedingly scarce. Dr. Dall has given in his paper (*Bull. Phil. Soc. Washington*, vol. xii., 1893, pp. 215–240) full and exact descriptions of these separate parts and their contents, and has thus rendered a service to conchologists, after an amount of labour known to few. The paper gives an excellent idea of the difficulties met with in determining and establishing an exact nomenclature when dealing with publications the dates and history of which are involved and obscure. The author hopes to be able shortly to reprint both of Conrad's works. Mr. G. D. Harris, of the Smithsonian Institution, is publishing a reprint of "Fossil Shells of the Tertiary Formations," at three dollars a copy, and invites subscriptions.

We have received the first fascicule of Fernand Priem's "La Terre" (Ballière, Paris), a book which deals with Seas and Continents, Physical Geography, Geology, and Mineralogy. This is, of course, a more or less popular account of the subjects mentioned, but the numerous and excellent engravings, which appear to be chiefly taken from photographs, make it an exceptionally useful book for the student. It will be completed in four parts, at two francs fifty centimes each, and will contain 700 figures.

Senhor Cazurro concludes his elaborate study of the actinian *Anemonia sulcata* in the third part of volume xxi. of the *Anales Soc. Espan. Hist. Nat.* The structure of this form is worked out in the most elaborate detail, and important light has thus been thrown on the group. In the same number, Westerland prints a "Faunula Molluscorum Hispalensis," and Girard a paper on "Céphalopodes des côtes de l'Espagne."

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

We learn that the Plymouth Marine Biological Station has lost the services of Mr. Calderwood, who has resigned.

Dr. Preuss, at the instance of the German Foreign Society, has gone to Victoria, in the Cameroons, as director of the Botanical Gardens. (*Bot. Centralblatt*).

Sir Henry Tichborne, who was attached to the Villiers Expedition for the exploration of Lake Rudolph, has returned to England in order to fulfil his duties as Sheriff.

Dr. William King, Director of the Geological Survey of India, has been granted an extension of service to July, 1894, notwithstanding the working of the 55 years' service rule.

Dr. D. Riva who took part in Professor Schweinfurth's last expedition to Eritrea, has undertaken a journey of exploration in the Ginba River district, East Africa. (*Bot. Centralblatt.*)

The fourth Report of the Epsom College Natural History Society, for 1892 has just appeared, and we see that the study of Natural Science is well appreciated in the school. The field-notes relate principally to dates of first bloom of flowers, Lepidoptera, first appearance of birds and dates of nesting, and meteorological observations.

Among the additions to the Herbarium of the Missouri Botanical Garden during 1892, we find mention of about 3,000 duplicates from the Herbarium of the late John Ball. The institution is also rejoicing in the donation by Dr. Sturtevant of his entire botanical library, which, according to the *Botanical Gazette*, "is undoubtedly the most complete and valuable American collection of pre-Linnæan botanical books."

On Monday, 27th February, the "Malacological Society of London" was founded, at a meeting held, by the kind permission of Mr. G. F. Harris, at 67 Chancery Lane. Mr. W. H. Hudleston took the chair, and the meeting elected Dr. Henry Woodward as the first president, Messrs. Hudleston, Melville, Godwin-Austen, and E. A. Smith as Vice-Presidents, Mr. E. R. Sykes as Secretary, Mr. G. F. Harris as Treasurer, and the following as its first council:—H. W. Burrows, J. H. Ponsonby, G. C. Crick, W. Crouch, Rev. A. M. Norman, B. B. Woodward and G. B. Sowerby.

A list of seventy names was read by the Secretary of those who had expressed their willingness to join the Society, and it was resolved that they be the original members. The annual subscription was fixed at half-a-guinea, and the entrance fee at the same sum; all those joining before December 31, 1893, will be elected by ballot in the customary manner, but will be exempt from the entrance fee.

The meetings of the new Society will be held on the second Friday in each month (November to June) at 8 p.m., and, for the present, Mr. Harris has courteously permitted the Society to meet at 67 Chancery Lane. The Council as at present appointed will draw up the rules of the Society, which will be presented to the first General Meeting, to be held on April 14. Letters to the Secretary (Mr. E. R. Sykes) should be addressed to 13 Doughty Street, London, W.C.

The objects of the society will be to study Malacology in all its branches, both recent and fossil, anatomical and skeletal, and if we may judge from the list of names furnished to us as founders of the society, has a great future before it. We hope, however, that it will do real scientific work, and leave the publication of local lists and small matters of that kind to the field clubs.

The Geologists' Association will start for Norwich on Thursday evening, March 30. On Friday the members will visit the Norwich Crag and Chalk at Bramerton and Thorpe, on Saturday the cliffs at Mundesley, Trimingham, and Overstrand and on Monday those from Cromer to Sherringham and Weybourn. On Tuesday, those who have the time still free will proceed to Lowestoft and visit the Pakefield Cliffs.

We have received from Professor Jules Marcou a pamphlet, entitled "A Little More Light on the United States Geological Survey," containing bitter personal attacks on many of the geologists. We do not profess to understand the rights of these personal disputes, but are sorry to see, from various papers that have lately reached us, that party feeling is affecting the value of much of the geological work done in the United States, both by the official geologists and those outside. Some comments on the work of the United States Geological Survey will be found in our last volume (p. 644).

Circulars from the World's Fair at Chicago arrive with startling rapidity. The latest we have received comes from the "General Division of African Ethnology," and unfolds a gigantic programme of work to be accomplished, dealing with Geography, History, Arts, Language and Literature, Religion, Natural Science, and Social and Political Science. Natural Science, of course, interests us the most, and we read that the following papers are promised:—"Astronomy," by W. W. Payne; "Structure of Africa and its Geological Systems," by James Geikie (invited); "Economic Geology of Africa," by Jos. Thomson (invited); "African Anthropology and Ethnology," by Heli Chatelain; "African Flora and Fauna, with Economic Botany and Zoology," by G. Schweinfurth and E. Holub (both invited). What the word "invited" means we are not told, but we only hope the desires of the promoters will be realised. There is a long and important list of "Advisory Council" printed at the end, and among those who are representatives of the different countries of the world, we read "H. M. Stanley, representing Humanity," and this entry comes first.

A systematic and alphabetical index of new species of North American flowering plants and ferns published in 1892 is in preparation at the United States National Herbarium. What a boon to systematists if all national Herbaria would follow this example!

A PRELIMINARY circular announces the organisation of a botanical survey of Nebraska, to be conducted by the Botanical Seminar of the State University at the expense of the members. Systematic botany evidently holds a far higher position across the Atlantic than in our English Universities.

PROFESSOR HERMANN CREDNER, of Leipzig, has issued a pamphlet, in English, explaining the methods and rate of progress in the Geological Survey of Saxony, of which he is Director. We observe that the greater part of the maps are already published, and that two or three years hence we shall see the completion of the Survey on the scale of 1 : 25,000.

ACCORDING to the *Botanical Gazette*, Professor Bolly, of the State University at Fargo, will exhibit at the World's Fair jars containing the tubercle-bearing roots of about forty species of North Dakota leguminous plants. The monumental work by Engler and Prantl (the progress of which, by the way, has somewhat slowed down of late), "Die Natürlichen Pflanzenfamilien," will be specially displayed by the publisher, W. Engelmann, of Leipzig.

FROM the same journal we learn that Beloit College has recently dedicated a new building, known as "Pearson's Hall of Science," in which admirably arranged botanical laboratories find a conspicuous place.

PROFESSOR BURDON SANDERSON, F.R.S., has been nominated to preside at the Nottingham meeting of the British Association. The following gentlemen have also been nominated to act as presidents of sections at Nottingham:—Section A, Mathematical and Physical Science, Professor Clifton, F.R.S.; Section B, Chemistry and Mineralogy, Professor J. Emerson Reynolds, F.R.S.; Section C, Geology, Mr. J. J. H. Teall, F.R.S.; Section D, Biology, the Rev. Canon Tristram, F.R.S.; Section E, Geography, Mr. Henry Seebohm, Sec. R.G.S.; Section F, Economic Science and Statistics, Professor J. S. Nicholson; Section G, Mechanical Science, Mr. Jeremiah Head; and Section H, Anthropology, Dr. Robert Munro.

Indian Engineering for February 18th has some remarks on "The Directorship of the Indian Geological Survey," from which we reprint the following:—"The Geological Survey has a lot of stiff geology before it yet in India which should be cleared off; though, as a matter of fact, the present director has been obliged to keep this geological progress back, in deference to the wishes of the Government to have mineral areas more thoroughly explored, and this doubtless goes against his reputation with his brother Geological Directors in Europe and America." The writer does not at all like the idea of "amalgamating the small scientific departments under a covenanted civilian," and prays, in view of such a contingency, for a special Mining Department. He goes on to say "The Geological Survey has been very judiciously kept away from the latter [gold and other mines]; though we are not so satisfied about the Department being kept, as appears to be the general policy, from making a proper *survey* of the gold and other mineral areas."

THE *Report of the Meeting of the British Association*, held in Edinburgh last year, has just been issued. Among the reports of the Committees we notice the following, which may be of interest to our readers:—12th on the "Earthquake and Volcanic Phenomena of Japan"; 19th on the "Rate of Increase of Underground Temperature downwards in various localities of Dry Land and under Water"; 18th on the "Circulation of Underground Waters"; 20th on "Erratic Blocks"; 3rd of the Committee to arrange for the "Collection, Preservation, and Systematic Registration of Photographs of Geological Interest in the United Kingdom"; final

report on "Cretaceous Polyzoa"; "Volcanic Phenomena of Vesuvius"; Zoological Station at Naples"; 5th on the "Zoology and Botany of the West India Islands"; 2nd on "Biological Association of Plymouth"; 6th on "Botanical Laboratory at Peradeniya, Ceylon"; "Teaching of Science in Elementary Schools."

The papers read in the various Sections are mostly published in short abstract, and have appeared in a more complete form elsewhere. Amongst the geological papers of interest, and containing original observations, we note one by Mr Peach "On a Widespread Radiolarian Chert of Arenig Age, from the Southern Uplands of Scotland," and another by Mr. Horne, "On the Contact Metamorphism of the Radiolarian Chert in the Lower Silurian Rocks along the Margin of the Loch Doon Granite." Mr. Dugald Bell criticises the "Alleged Proofs of Submergence in Scotland during the Glacial Epoch"; Mr. Clement Reid gives a list of the "Fossil Arctic Plants found near Edinburgh"; and Messrs. Peach and Horne write on "The Ice-Sheet in the North-West Highlands during the Maximum Glaciation," and on a "Bone Cave in the Cambrian Limestone in Assynt, Sutherlandshire." The remains found in the cave at Assynt seem to be of Recent or Neolithic date.

In the section for Biology, the chief botanical papers are Dr. Goebel's "On the simplest form of Mosses (*Buxbaumia*)"; George Murray's "On a comparison of the Marine Floras of the warm Atlantic and the Indian Ocean"; H. W. T. Wager, "On the structure of *Cystopus candidus*," the parasitic fungus of the shepherd's-purse; and Gustav Mann's, on "The Embryo-sac of Angiosperms." The papers of most interest in Zoology are those of E. W. Carlier, "On the structure of the so-called Hibernating Gland in the Hedgehog"; and "On the Skin of the Hedgehog"; of Gustav Fritsch, "On the origin of the Electric Nerves in the *Torpedo*, *Gymnotus*, *Mormyrus*, and *Malapterurus*. Several interesting economic papers on Fisheries—Calderwood, "On the Destruction of Immature Fish"; Holt, "On the Relation of Size to Sexual Maturity,' and "On the Destruction of Immature Fish in the North Sea," and Cunningham "On the Protection of Immature Fish." Of the purely Physiological papers, the most interesting are—Marcus Hartog, "On Rabl's Doctrine of the Personality of the Segments of the Nucleus," and Weismann's "'Idant' Theory of Heredity"; and Gustav Mann, "On the Origin of Sex,"

A Guide to Sowerby's Models of English Fungi in the Department of Botany has been published by the Trustees of the British Museum. It has been prepared by Mr. Worthington Smith, who, a few years ago, restored the models to their original colour. The little book of 82 pages, with 93 illustrations, costs only fourpence, and deserves particular notice from the fact that it is not merely a guide to the models, but to the larger forms of our native fungi. All the prominent genera of the *Hymenomycetes*, *Gasteromycetes*, and *Ascomycetes*—prominent from their size—are represented here in their typical species with adequate descriptions, and a woodcut for each genus. The so-called microscopic forms are not dealt with, and since their study is not by any means so popular as that of the larger fungi, there will be no popular regrets on this head. The search for, naming, and drawing of mushrooms and toadstools is a very favourite pastime, apart from its scientific interest, among large numbers of leisured country folks, as well as the Saturday afternoon naturalists, and the issue of this little book for fourpence, with its well-executed figures and simple descriptions, is sure to be popular, apart from its interest as a guide to the models. In fact it is much more likely to be utilised away from the models than in presence of them. Mr. Worthington Smith's magnificent series of drawings exhibited beside the models is another attraction to the botanical gallery for students of Fungi, and the authorities are to be congratulated on the exhibition, as well as on the production of this Guide, which bears all the marks of honest work.

OBITUARY.

KARL AUGUST LOSSEN.

Died February 24, 1893.

SCIENCE has sustained a severe loss in the death, on February 24, of Dr. Karl August Lossen, Professor of Geology at the School of Mines and at the University of Berlin, and chief Geologist of the Prussian Survey.

Dr. Lossen was an enthusiastic field geologist and an able petrologist. Most of his work was done in the Hartz, and his well-known map of that district is remarkable both for its accuracy and for the minuteness of the detail. He was one of the first to establish the immense importance of dynamic agencies in modifying the structure and composition of both igneous and sedimentary rocks, and he clearly recognised that the principles which he had established by detailed work in his own area were applicable to all parts of the Hercynian range in its course through Europe, from the West of England to the Sudetes. His early papers will be found in the *Zeitschift der deutschen geologischen Gesellschaft*, between the years 1864 and 1880. His later and, in some respects, most important work was published in the *Jahrbuch der königlich preussichen geologischen Landesanstalt*, and in the detailed memoirs on the separate sheets issued by the Prussian Geological Survey (1 : 25,000).

The great importance of his work has failed to attract the general attention which it deserves in consequence of his extremely involved literary style—a style which was probably due in part to his unfortunate deafness. All those who had the good fortune to meet him speak with enthusiasm of his charming personal character, and of his extreme readiness to communicate any information which he possessed. He was a Foreign Correspondent of the Geological Society of London.

WE have also to announce the death of Professor Schaaffhausen, of Bonn, whose name was prominently before the scientific world some thirty years ago in connection with the famous Neanderthal Skull. In 1880 he visited this country, bringing with him the original skull, which he exhibited and described at the Swansea meeting of the British Association. He was born in 1816.

WE learn with regret that Professor Karl Prantl died on February 24, at Breslau, where he was Professor of Botany and Director of the Botanical Gardens. He is, perhaps, best known in this country for his "Lehrbuch," of which an edited translation by Professor Vines has been widely used among students, who, while in need of a modern text-book, found that of Sachs somewhat too ponderous. He was also joint editor, with Professor Engler, of the valuable "Pflanzenfamilien," still in progress of issue, and sole editor of *Hedwigia*, the organ of those who love mosses. Professor Prantl was also the author of some Researches on the Morphology of Vascular Cryptogams, and an "Exkursionsflora fur das konigreich Bayern." Last year he started a new publication, entitled "Arbeiten aus dem Kon. Bot. Garten zu Breslau."

DEVONSHIRE has lost an enthusiastic and observant naturalist in the person of Edward Parfitt. Born near Norwich in 1820, the son of a gardener, he had from his earliest youth a passion for studying life of all kinds, which led him to go to sea in order to get some acquaintance with foreign animals. Wrecked near the Cape, he was obliged to make a long stay which increased his taste for Botany and Entomology, and allowed him to make a collection. On his return to England he devoted himself to Horticulture, and went to Devon about 1846. Since then Parfitt worked on the Natural History of the County, published numerous local papers, and has left a MS. on the Devon Fungi in 12 volumes, illustrated by 1530 plates, drawn and painted by himself. He was Librarian to the Devon and Exeter Institution for 32 years. Parfitt was entirely a self-made man, and Devon has lost a naturalist she could ill spare. He died on January 15.

TO CORRESPONDENTS.

All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

All communications for the Publishers *to be addressed to* Macmillan & Co., 29 *Bedford Street, Strand, London, W.C.*

All Advertisements *to be forwarded to the sole agents,* John Haddon & Co., *Bouverie House, Salisbury Square, Fleet Street, London, E.C.*

ERRATUM.

P. 207, line 8, for "elder" read "alder."

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NOTES AND COMMENTS.

The Pollination of the Yucca.

MR. A. W. BENNETT, in his article in the March number of Natural Science, refers to the pollination of the Yucca, a well-known liliaceous plant, by a moth (*Pronuba yuccasella*), and those interested in the subject will find some "Further Studies," by William Trelease in the fourth annual report of the Missouri Botanical Garden. Hitherto, only two species, *filamentosa* and *glauca*, have been directly observed under cultivation, and the latter wild in Colorado, and the moth *Pronuba* is the pollinating agent in both. The present report describes further investigations in the field on these and other species.

The flowers of *Yucca baccata*, a widely distributed species, ranging in a variety of forms from southern Colorado into Mexico and California, are frequented by a moth somewhat larger and a little darker in colour than the *P. yuccasella* of the Mississippi Valley and Rocky Mountains, but otherwise indistinguishable from it, as is also the moth of the Gulf region. Like the Eastern representative, it rests within the flower during the day with the head directed towards the base of the stamens, and when depositing the egg, at night, "doubtless backs down between the upper ends of the stamens," piercing the ovary at about its middle in the thinnest part, viz., that between the internal partitions, which is not covered by the lower part of the stamen. "After thus depositing the egg in the ovule of the plant, the females behave as in *Y. filamentosa*, carrying loads of pollen from the anthers and thrusting it into the cavity of the stigma with the proboscis. This open canal is frequented by numbers of a white Thrips, which sometimes penetrate the cells of the ovary and doubt-

less assist fertilisation by scattering the pollen. Dried fruits of the previous season showed the perforations made by the escaping larvæ.

Yucca australis, studied in south-west Texas, shows seeds tunnelled in the manner characteristic of the moth, the pulp being perforated by the escaped larvæ. No flowers were observed, but large fruits, gathered some three weeks after fertilisation, bore none of the constrictions or indentations which so commonly mark the puncture of the moth, a fact which suggests that the eggs are deposited in the upper part near the stigma, as was found to be the case in the large tree yucca, *Y. brevifolia*. The agent in the flowers of the latter is the dark-coloured *Pronuba synthetica*, which is more active through the day than its eastern congener, and, unlike the other known species, slow to take flight.

This apparent disinclination to leave the flowers may, perhaps, be connected with the almost constant occurrence of high winds in the desert, and may restrict cross-fertilisation to flowers of the same plant more closely than in other Yuccas, though there must be frequent flights from plant to plant in quiet weather, and especially at night, when the wind sometimes falls. Moreover, the development of the stigma two days before the stamens of any one flower renders close fertilisation, in the strictest sense, impossible. Owing to the light yellow colour of the pollen, in marked contrast with the smoky tint of the moth, laden females can be seen from a considerable distance.

The female of most species of *Pronuba* seeks a fresh flower wherein to lay her eggs, preferring one in the first night of expansion, doubtless to ensure for her offspring a sufficient food-supply, the younger flowers being less likely to be already overstocked with eggs. This instinct is especially marked in *P. synthetica*, which was never seen to use any but the youngest flowers, while pollen-laden moths were repeatedly noticed forcing their way in the very narrow clefts between the rigid sepals of an opening bud, their flattened form facilitating this. When about to deposit an egg, the female runs to the bottom of the stamens much as *yuccasella* does, makes a rapid, more or less complete, circuit of their bases, and then quickly mounts to the very top of the pistil, and with her short, strong ovipostor cuts through the thin wall into the channel of the style close below the tip of the stigma, holding fast to the pistil meanwhile, the stamens being below her reach. The long extensile oviduct is then passed through the puncture, the egg being laid apparently within the ovary cell. The operation usually takes from two-and-a-half to three minutes longer than in other observed species. Sometimes two or more eggs are laid before the stigma is pollinated, but commonly, after laying each egg, the moth retreats to the bottom of the flower, and then again mounts the pistil till her head is even with the stigma, when she uncoils the large tentacles from their resting-place against her load of pollen and passes them back and forth in the stigmatic chamber with almost the same motion as *yuccasella*. The deposition

of the egg just below the stigma explains the remarkable symmetry of most of the fertilised pistils, contrasting with the constriction or indentation of the Eastern species at the point where the ovary has been punctured.

In another smaller species, *Yucca elata*, observed at Eagle Flat, Texas, *Pronuba yuccasella* was the agent of pollination. When about to deposit an egg, the moth, as in *Y. filamentosa*, runs nervously about within the bottom of the flower, then scrambles to the top of the pistil and backs down between two stamens by a succession of jerks, until her head is about level with the base of the style. Clinging to the pistil, she then punctures the ovary, and the egg is consigned to its place in the manner so well described by Riley for *filamentosa*. Usually each oviposition is followed by pollination, but in a few cases two eggs were laid before pollen was carried to the stigma, and sometimes so disturbed is the moth by the bright light of the observer's lantern, that she will leave a flower altogether without pollination. In one such instance her first act on entering another flower was to thrust her pollen-laden tentacles into the stigma, though it is unusual for this to precede oviposition. On several occasions, when disturbed in laying her egg, the moth ran upon a stamen, shaking it quite violently and making several passes at the anther with her tentacles, as if frightened from one of her occupations only to engage in another. The very distinct Californian *Yucca Whipplei*, as Professor Riley has shown, is pollinated by a distinct *Pronuba*, which he names *P. maculata*, of a general white colour, but mottled with black. In this species the pollen is not loose and powdery, as in Yucca proper, but glutinous, and frequent observation has shown that it may be deposited on the stigma directly from the anthers in closing flowers, so that self-pollination becomes possible, and Mr. Coquillet, of Los Angeles, records the seeding, in 1892, of a number of panicles which he had covered with gauze before any of the flowers opened. Owing to the more open and more diurnal character of the flowers in this species, the moth is far more active in the daytime than its congeners. Moreover, the resting position, with the head towards the stigma, is almost identical with that taken in oviposition, which, like pollination, may be witnessed at any time during the day. Standing on the side of the pistil the female pierces the ovary at the thinnest part. Generally not more than six eggs are laid in one pistil—one on either side of each septum—often fewer, so that even if they all hatch—an unlikely event—there is rarely more than one larva to each tier of seeds, thus allowing a fair percentage to ripen. Pollination was well seen in the variety *graminifolia*, where it was observed several times under a cloudless sky, the first time at about noon. Flying into a flower, the moth runs about the bases of the stamens in the usual way, then quickly clambers upon the inner side of a filament, and, with the tentacles stretched over the pollen masses, drags first one and then the other out of the anther cells, pressing

them together under the throat, and subsequently working them into a compact mass, much as *yuccasella* does the powdery pollen of other yuccas. The stamens are often quite violently shaken, so quick and energetic are the movements of the moth.

As the author suggests, the relationship of the Yuccas to one another and to the Pronuba moths would be far more intelligible could we trace their history, even a short way back, into geological time, but as no certainly identifiable Yuccas exist in even the latest deposits, this is as yet impossible.

He thinks it reasonable to suppose that an ancient type, represented in the tree-like *Y. brevifolia*, with an equally ancient type of Pronuba, has persisted in the Pacific region, perhaps, for the same reasons that have led to the preservation of the gigantic Sequoias in the same region, while the more widely distributed species have become differentiated, and, with their pollinator, passed to the south under new conditions.

THE "ZOOLOGICAL RECORD."

IN the Report of Accounts of the Zoological Society for 1892, there is an item of £937 for "cost of *Zoological Record*." The same entry for 1890 amounted to £650, and for 1891 to £420. There is no statement as to the reason of this enormous increase—a somewhat unfortunate oversight on the part of those who drew up the Report—and it is well that the matter should be explained. The £937 not only includes the *Record* for 1891, but includes a great part of the *Record* for 1890. The "sales" for this invaluable compilation were, in 1890, £298; in 1891, £266; in 1892, £354; a net improvement in the last year of the three of £56.

It seems rather difficult to believe that a volume like the *Zoological Record* should only enjoy the ridiculous sale of barely 250 copies; and it is certainly most praiseworthy that the Zoological Society, despite the annual loss, refuses to allow the good work it is doing to fall to the ground. Surely there is no museum or laboratory in existence that can afford to neglect zoological literature to such an extent as to refuse to subscribe to the *Record*; and yet it is hard to believe that there are only 250 museums or libraries of repute in the whole world. There is certainly no other Record that is anything like so up-to-date as the Zoological Society's publication. It has always been a matter of regret that some amicable understanding has not been arrived at by the Royal, the Linnean, the Geological, and the Zoological Societies which will allow the proper listing and publication annually of a complete literature of the animal, mineral, and vegetable kingdoms. The Zoological Society, despite the enormous outlay required by the Gardens, cheerfully bears the whole of the expense of providing students of zoology with

an efficient yearly catalogue, and sets a worthy example to its sister societies, with their accumulated and accumulating capitals, and the additional benefit enjoyed by them of rent-free premises in Burlington House. We look for better things from scientific societies than heavy investments in the Funds.

Ice in Hong Kong.

The *Gardener's Chronicle* of April 8 gives some particulars of the unprecedented cold weather which occurred in Hong Kong last January. There is no previous record, says Mr. Ford, the Director of the Botanic Gardens, of ice occurring in the island below 1,700 feet, and hence, as it lies within the tropics, the result was most disastrous, indigenous, as well as cultivated, plants suffering.

"The continued low temperature," he says, "combined with a fall of rain from an apparently warmer stratum of air above, resulted in the formation of ice varying in quantity from a thin coating on the upper leaves of pine trees growing at 300 feet above sea-level, to a thick encasement of perfectly transparent solid ice of 5½ inches in circumference on the blades and bents of grass at the summit of Victoria Peak." Leaves of evergreen shrubs and trees bore solid coverings of ice three-eighths of an inch in thickness, the great weight of which caused the branches to assume a pendent form, and in many cases snapped off the limbs. All vegetation throughout the hill regions of the colony was covered in this way with ice, as were also most other objects. Telegraph wires had a casing more than half-an-inch thick, and bore icicles three inches long as close as they could be packed side by side, while the windward sides of the walls of the look-out at the peak were sheeted from top to bottom with a perfectly transparent coat three-quarters of an inch thick. All the hills on the mainland and Lantao Island were likewise white with ice.

The damage in the Gardens consists chiefly in injury to foliage, but some plants, natives of warmer regions, are quite killed, and many of the decorative plants which survived will be months before they reassume their ornamental appearance.

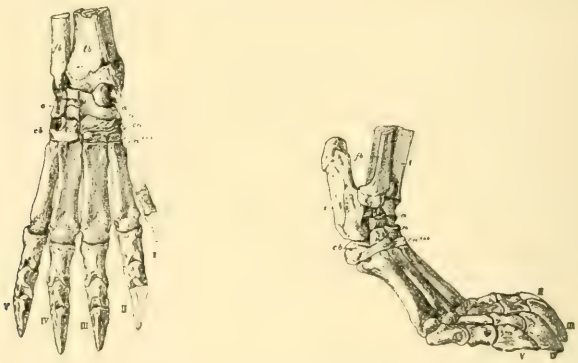
In spite of every possible precaution in matting-in the plant-houses and covering the roofs with straw, the contents suffered greatly, many of the best orchids being killed outright, and others so much injured that even if they survive it will be years before they regain their former luxuriance. It is interesting to note that, while a healthy plant of *Dendrobium aggregatum* received from Calcutta several years ago is apparently killed, plants of the same species growing by its side, and others on trees where they had no shelter, which were collected ten years ago on the Lo-fau mountains, about sixty miles from Canton, have escaped unharmed. Several exotic trees planted on the hills had all their leaves killed, while above 600

feet the indigenous vegetation was much injured, especially the tropical plants which here reach their northern limit.

Dr. Henry reports that the destruction of vegetation about Canton has also been very great. The banana plantations are ruined, the bamboos have suffered, the candle-nut trees (*Aleurites triloba*) are all shrivelled up, while begonias, euphorbias, crotons, and scores of others, are quite destroyed.

A Clawed Artiodactyle-like Mammal.

From several references to the subject, readers of Natural Science are probably familiar with the fact that the remarkable clawed Miocene and Pliocene mammal known as *Chalicotherium* presents many curious resemblances to the ordinary Perissodactyle Ungulates. Messrs. Osborn and Wortman now announce the discovery of another American Tertiary mammal, for which they propose the name *Artionyx*, with almost precisely similar resemblances to the Artiodactyle divi-



Front and hind foot of *Artionyx*.—(From Osborn.)

sion of the Ungulates. The authors refer both these curious creatures to a single group, which they suggest originated from the primitive Ungulates before the acquisition of distinctly Ungulate terminal phalanges; and they call attention to the curious double parallelism existing between these two forms on the one hand and the Artiodactyle and Perissodactyle Ungulates on the other. All this confirms the view which we have so frequently urged as to the impropriety of separating *Chalicotherium* as an order from the Ungulates.

Mountain Chains as Barriers.

Dr. J. Regnault, in a lecture before the Ethnographical section of the Geographical Society of Paris, has drawn attention to the "Influence of Mountain Chains on the Distribution of the Human Race." His original observations resulted from a visit to Darjeeling and the Terai, but his conclusions are drawn from a study of the

whole of Europe and Asia. He states the following laws :—(1) When a chain of mountains, such as the Alps, the Pyrenees, separates two nations, ... one generally thinks that the crest, the water-shed, divides two races, each inhabiting his respective slope up to the summit. Nothing of the kind, and one can say that the crest of a chain does not separate two races. In support of this statement, Regnault says :—This error is due to the fact that the crest generally serves as a frontier between two States. (2) Ordinarily the race of the country towards which the lesser slope inclines inhabits not only this slope, but crosses the crest and colonises the more rapidly-sloping side down to the plain in which the slope is lost. It is the junction between the rapid slope and the plain where the contact of the two races is to be found. To the second law he adds two postulates:—(*a*) The mountain chain must be high and long, as the Alps and Pyrenees; (*b*) The law is modified by considerations of time, geology, climate, &c. Dr. Regnault discusses in some detail the Alps, the Pyrenees, Erz-Gebirge, Caucasus, and the Himalaya. The lecture, which is highly interesting, will be found printed in *Revue de Géographie*, March, 1893.

Myeloxylon.

In the recently-issued number of the *Annals of Botany* (vol. vii., p. 25), A. C. Seward discusses the affinity of the fossil plant genus *Myeloxylon*. Corda thought it a Palm, Brongniart a doubtful Monocotyledon, while Goeppert referred it to those plants which must be looked upon as prototypes or synthetic types, recognising in its structure the Fern, Gymnosperm and Monocotyledon. Binney, Williamson, Renault, and Kidston make it a fern rachis of the order Marattiaceæ, while Schimper, Schenk, and Felix prefer to regard it as the petiole of a Cycad. The present account embodies the result of an examination of some new material from the Millstone Grit, and elsewhere, and also of some specimens now in the British Museum, and once the property of Robert Brown. The author has had the good fortune to get sections showing very well the structure of the vascular bundles, the form of which closely corresponds with that in Cycads of to-day, while the position of the first-formed wood vessels on the side of the wood next to the bast is also in striking agreement with these. The bordered pits, characteristic of the recent Cycads, were not seen, but there are points of resemblance between the wood elements in both, while the resin canals of recent Cycads and the fossil petioles are practically identical.

On the whole, the author concludes that the fossil specimens approach more nearly to the Cycads than the Ferns. The few points of difference which distinguish them from the petioles of recent Cycads are, he thinks, far outweighed by the close parallelism in more essential characters, and he looks upon *Myeloxylon* as an

extinct genus not corresponding exactly to any recent family of plants, but coming very near the Cycads in anatomical structure, and probably holding a position between Cycads and Ferns, but nearer to the former.

EMIN PASHA.

THE *Berliner Tageblatt* for April 5 publishes a letter from its special correspondent, dated Fort Kampala, Uganda, December 14, 1892, which tends to confirm the rumours of the death of Emin Pasha. According to the letter, news had reached Fort Kampala that Emin Pasha marched from Kavalli to Masamboni, and thence to the Ituri river, on the banks of which he was attacked by a body of Manyema and killed. The correspondent of the *Tageblatt* further reports having met an Egyptian official, named Awad Effendi, on his way to the coast, who informed him that he was present with Emin in Masamboni's country, and added that Emin Pasha and all his followers had been murdered on the Ituri river by Manyema, under the leadership of an Arab named Ismail. Awad Effendi believed that the murder was committed on March 12 or 13, 1892.—*Reuter*. This may be another version of the rumour to which we referred in July; at present, we cannot be sure of the accuracy of the information.

THE AFFINITIES OF ZEUGLODON.

IN the last issue of the *Proc. Zool. Soc.*, Mr. Lydekker describes an interesting series of Cetacean remains from the Eocene of the Caucasus. Among these, special importance attaches to certain bones belonging to the imperfectly-known creature designated *Zeuglodon*, which, as our readers are doubtless aware, Professor D'Arcy Thompson has recently endeavoured to remove from its assigned position among the whales to associate it with the seals. The most important specimen among the new find is a humerus, of which but one example has been hitherto known, and the study of this leads the author to conclude that *Zeuglodon* is much more likely to be an ancestral Cetacean than a Seal. Doubtless, however, Professor Thompson will have a word to say on the subject. Scarcely less interesting is the discovery of a skull in the same deposits, indicating a Cetacean nearly allied to the existing *Inia* and *Pontoporia* of the South American rivers. We regret that the artist has scarcely done justice to the author's specimens.

NEW MONKEYS.

IT is probably a unique feature in a single number of the serial just mentioned to find three new species of monkeys described and illustrated with coloured plates. Two of these belong to the Oriental genus *Semnopithecus*, and the third to the Ethiopian *Cercopithecus*.

In regard to one of those belonging to the former genus, the describer observes that specimens may subsequently show a transition from the supposed new form to one which he has previously named *S. hosei*, in which case the former will have to be reduced to the rank of a variety. Nevertheless, he adds, "in the absence of such intermediate forms, and in view of the great constancy in the coloration of *S. hosei* already noted, it seems best to give a name to the striking variation from it now described." We venture to have doubts as to the wisdom of this course.

Pictures at Brighton.

It is not often that Art and Science go hand in hand, therefore two collections of pictures now exhibited on loan at the Brighton Corporation Art Gallery are of an unusually interesting and instructive character, from a scientific point of view. The series of African scenes, lent by Mr. Robert White, painted in oils by Mr. Thomas Baines, the well-known explorer, illustrate the geology, zoology, botany, and ethnology of the South and West regions of that continent. The successive river-terraces of the Great Zambesi, and its winding, deep-cut, cañon-like gorge, are well depicted by the artist, a brother of Mr. Geddes Baines, discoverer of the Dicynodonts. There is a picture of the spot where remains of one of those extraordinary reptiles were first found. Miss C. F. Gordon Cumming exhibits a series of water-colour sketches of Ceylon and Fiji, including a fine set showing the river-terraces, silica-deposits, sulphur, mud, and hot springs of the volcanic district of New Zealand, where the whole aspects of the region were so completely changed by a subsequent convulsion of Nature, that these pictures portray scenes no longer in existence. The luxuriant flora of Ceylon is well illustrated, and there is a capital sketch of the sacred Singalese relic "Buddha's tooth," whether "shed" or "removed" tradition is silent.

Fauna of the Plöner-See.

We are glad to welcome the first number of the *Forschungsberichte aus der Biologischen Station zu Plön* (Berlin: Friedländer), by Otto Zacharias, the Director. The number opens, as it should do, with a list of the Fauna of the great Plöner-See, the compilation of which has brought to light many new forms, all duly described and figured in the succeeding pages. These include a Rhizopod, a Heliozoan, a Mastigophore, an Infusorian, a Turbellarian, and eight Rotifers. If so good a result is achieved in making up a first list, we may expect a large addition to our knowledge of fresh-water life from the work done at the Biological Station at Plön.

THE PEOPLING OF THE WORLD.

THE last of those valuable "Ergänzungshefte" (number 107), published at intervals by the proprietors of *Petermann's Mitteilungen*, is devoted to the "Peopling of the World." Drs. Hermann Wagner and Alexander Supan have to be thanked for the prodigious labour involved in the compilation of this great census, the mass of figures in which is simply appalling. The number consists of 130 pp., and is crammed with information. The continents are taken in order, and, whenever possible, the census of the actual town is given in 1891. Details of the previous census are also given in each case, so that the reader may see at a glance the increase of population in a certain number of years. Towards the end comes a list of the large cities of the world and their inhabitants, and we extract the first six as having special interest:—

London (1891)	4,415,958
Paris (1891)	2,712,598
New York and Brooklyn (1890)	2,352,150
Berlin (1890)	1,763,543
Canton (1891)	1,600,000
Vienna (1890)	1,364,548

There are only 12 cities with over a million inhabitants, of which China rejoices in possessing four.

FRESH-WATER MEDUSÆ.

SPECIMENS of the fresh-water Medusa from Lake Tanganyika, the same probably as that referred to by Böhm in 1883, and Wissmann in 1887, have been received in good condition, and form the subject of a paper by Mr. R. T. Günther in the April number of the *Annals and Magazine of Nat. Hist.* The individuals vary in size from 1 to 1·8c. across the bell, while the largest measured as much as 2·2c. The umbrella is flattened; the tentacles very numerous, varying in length, and arranged to a sixth order with great regularity. Mr. Günther establishes a new genus, *Limnocnida*, for this interesting animal, and preserves Böhm's name of *tanganjicæ* for the species, that author having recognised the form as new, although he was unable to describe it in 1883 for want of the necessary literature. The fresh-water Medusa which thrives in certain years in the tank at the Botanic Gardens, Regent's Park, *Limnocodium sowerbii*, differs considerably from this new form.

THE PLAGUE OF VOLES.

WE have referred on several occasions to the subject of the plague of Field-Voles in Scotland, and we shall, therefore, merely chronicle the publication of the "Report of the Departmental Committee on Field-Voles (Scotland)," Parl. Paper—C.—6943, 1893, price

1s. 4d., which appeared early in the month. The Report traces the nature and origin of the plague; causes of the outbreak; effect on the pasture and on the stock; remedies; previous outbreaks, among which the Philistines and other biblical stories are quoted; natural enemies of the voles; conclusions and recommendations. Of course, the obvious remedy, and one which is hinted at in the Report, is the non-destruction of the birds and mammals that destroy the voles for food; but it is almost useless to insist on the preservation of the balance of life in Nature where the game-breeder and game-keeper are concerned. The owls, buzzards, and kestrels, the stoats, weasels, and foxes, are sacrificed to the game-god, and therefore the sacrificers bring other (and greater) evils on their own heads and on those of others. The Report is illustrated by figures of voles and their natural enemies.

A New Variety of the Pala.

In the last number of the *Proc. Zool. Soc.*, Mr. Thomas describes a new variety of antelope differing from the ordinary Pala by its much smaller horns and more slender skull recently obtained from Nyassa-land. The especial interest attaching to this form is that it occupies an "island" within the distributional area of the common Pala.

Costa Rica.

An excellent map of Costa Rica will be found in "Geografia de Costa Rica," by F. M. Barrantes (*Barcelona*, 1892). This work, which has been occasioned by the Exhibitions of Madrid and Chicago, gives a sketch of the territory, its limits and coast-lines, mountain and river systems, climate and productions. A list follows, of the birds, taken from Zeledon's papers, reptiles and fishes, and mammals. This last is a translation of Frantzins' paper, and occupies 72 pages. It is followed by a series of chapters on population, government, and religion, and then the book deals with the various provinces and islands. There are a great number of full-page illustrations, reproductions of photographs of town scenes chiefly, and one of the crater of the volcano Poas, which is most interesting, being taken from the edge, and showing the crater-lake. There is no index.

The Origin of the Dingo.

In a useful descriptive catalogue of the Mammals of Australia, recently issued by the Australian Museum, Sydney, Mr. J. D. Ogilby enters very fully into the disputed question of the origin of the Dingo, or native dog. After quoting the chief recent authorities who have written on the subject—more especially the late Professor McCoy—

the author observes that, in regard to the question whether the dingo "is an indigene or has been brought hither through human instrumentality, we consider, notwithstanding that the greater number of authors incline to the latter theory, that the recognition by Professor McCoy of fossil remains, in no wise differing from those of recent individuals, and contemporaneous with similar remains of *Thylacoles*, *Diprotodon*, &c., sets the question at rest, and goes far towards proving that the species is indigenous to continental Australia, and was an inhabitant thereof prior to its colonisation by man, no human remains of such antiquity having as yet been discovered." This conclusion is supported by the recent discovery of dingo remains in a stratum lying beneath 63 feet of volcanic sand, underlain by another 60 feet of blue and yellow clay, thus clearly indicating the great antiquity of the animal in question.

From our own experience of the specimens in the British Museum, we have hitherto believed that remains of the dingo only occurred in the Australian cavern-deposits, where, we believe, human remains also occur; but these new discoveries seem to indicate the existence of this dog at a period when it is almost impossible to conceive that man had reached Australia. Startling as it may seem, we are, therefore, inclined to believe that the dingo may really be an indigenous Australian mammal. There are, indeed, several indications that we shall ere long have to revise our views as to the origin of the mammalian fauna of Australia; and the number of placental land mammals indigenous to that country noticed in the catalogue before us shows how incorrect it is to speak of Australia as exclusively a land of Marsupials and Monotremes.

Admitting that Mr. Ogilby may very probably be right as to the indigenous origin of the dingo, we can, however, scarcely follow him when he states that "until proof to the contrary is forthcoming, we shall consider the honour of being the original progenitor of our household favourite as the due of the Australian warrigal" (another name for the dingo). Surely the author is aware that in Europe the remains of dogs closely allied to the wolf occur in Pliocene beds, and that there is every reason to believe that the Eskimo dog, as well as sheep-dogs, are more or less directly descended from the wolf, while the Hare Indian dog of North America is just as closely related to the coyoté. The origin of the numerous breeds of domestic dogs cannot, indeed, be disposed of in this off-hand manner.

Nesting of the Gobies.

A paper, by F. Guitel, which has appeared in the last number of Professor de Lacaze-Duthiers's *Archives de Zoologie expérimentale*, contains interesting notes on the hitherto little-observed habits of one of our commonest coast-fishes: *Gobius minutus*. The Gobies belong to the few fishes which construct a nest for the shelter of

their eggs, a work which, as in our Sticklebacks, devolves on the male parent alone. Although the fact of Gobies making nests has been known since the time of Rondelet (1554), no sufficiently-detailed account of the breeding habits of any species of the genus had yet appeared. The male *Gobius minutus*, for the nest, in tidal pools chooses usually the shell of some bivalve, frequently the Cockle, under the concave surface of which the sand is hollowed out and cemented by a special mucilaginous secretion from the skin of the fish ; a cylindrical tunnel gives access to the nest, which is covered over with loose sand. The female having deposited her eggs, fixed to the shell, the male watches over them, and courageously defends his nest during the whole period of incubation, lasting from six to nine days.

The Oligodynamics of Living Cells.

A posthumous paper of Carl von Nägeli has just been published at Zurich, with the mysterious title "Ueber oligodynamische Erscheinungen in lebenden Zellen."

Towards the end of the discussion, we read that living cells may succumb to three different forces. Of these the Physical and Chemical are by far the most frequent and fatal ; a third, of comparatively rare occurrence, he chooses to term "Oligodynamic." The paper is occupied by an examination of this peculiar power.

The first startling announcement runs "fatal effect of nominally pure water on living cells." Nägeli was led to the conclusion that water possessed this property while testing the reduction of silver salts by living protoplasm. Solutions of the silver salts were made in the "nominally" pure water, either distilled or spring water, and the cells experimented on were those of Spirogyra.

In strong solutions, he noticed that the effect was chemical, the plants dying from poison ; the cells lost their turgidity, the protoplasmic layer separated from the wall, and the chlorophyll bands changed colour.

With a solution so diluted that the merest infinitesimal trace of the salt only was present, the result was again death, but after a wholly different fashion ; the protoplasmic layer still adhered to the wall, and the cells retained their turgidity, while the chlorophyll bands shrunk together into a round ball. The effect was sometimes very rapid, and in four or five minutes the cell was killed. This, then, is the oligodynamic phenomenon, and, as he discovered at the end of a long research, was caused by molecules of copper present in the water.

The "nominally" pure water was brought through leaden pipes ending in a brass tap. If the water was allowed to flow some time before being drawn for use, it was neutral ; the experimenter could thus at will command oligodynamic or neutral water. In the case of

distilled water, the vessels employed in distillation were accountable for the fatal effects.

River, lake, or marsh waters were neutral, but were rendered oligodynamic by the immersion of coins, and again could be neutralised by the addition of some substance, such as sulphur, wool, cellulose, &c., which would expose a large surface, and so attract the copper molecules.

The paper is prefaced by Schwendener, who informs us that Nägeli intended to name the newly-discovered power Isagität, but later he changed it to Oligodynamic.

Professor Cramer, of Zurich, repeated the experiments, and in a most interesting paper gives us his results, which confirm those arrived at by Nägeli.

THERE is a brief and succinct account of the Sandgate disaster, by Mr. William Topley, in the April number of the *Geographical Journal*, which contains some curious references to earlier landslips in the same area, 1716, 1725, 1786. Mr. Topley delivered an interesting lecture on the subject to the Geologists' Association on April 7. A Report of this will be printed in the May number of the *Proceedings*.

IN the *American Geologist* for March, Charles Schuchert writes on the classification of the Brachiopoda. The genera (author and date quoted) are arranged in families, many of which are new, and there is one new genus, *Dallina* (Beecher), founded on *Waldheimia septigera*, Lovén. Following the classification, is a "Geological Distribution of the Brachiopoda," a most useful list, but which would have been of more service if arranged alphabetically, for the alphabet is stable, while systematists have their idiosyncrasies. Dr. C. E. Beecher has assisted Mr. Schuchert in his work.

WE have received, from the University Press of Chicago, the first number of a new "Semi-Quarterly Magazine of Geology and Related Sciences," called the *Journal of Geology*. It is excellently printed on hot-pressed paper, and has seven editors and thirteen associate editors. The first article is one on the Pre-Cambrian Rocks of the British Isles, contributed by Sir Archibald Geikie; most of the remaining space is devoted to questions relating to the glacial geology of America.

A PARAGRAPH has appeared in the daily Press reporting the discovery of coal in Essex. We understand that the report is without foundation, though it is by no means improbable that Coal-measures may occur at a moderate depth in that county.

The *Mediterranean Naturalist* for March contains several interesting papers relating to the district around Malta. Surgeon-Captain M. L. Hughes continues his "Natural History of certain Fevers occurring in the Mediterranean"; Gaetano Platania publishes some "Geological Notes of Acireale," and Dr. Alf Caruana Gatto writes on "The Vegetation of the House Terraces of Malta."

In the *Naturalist* for April Mr. John Cordeaux continues his "Bird-notes from the Humber District in the Winter of 1892–1893." The arctic severity of the winter led to a great rush of various species on the East Coast, all wild fowl, such as geese and duck, having been exceptionally abundant.

The same journal has a curious note by Mr. H. Moody Foster on herrings confined in a brackish-water pond communicating with the Humber. The fish are dwarfed, and are easily captured by rod and line.

Mr. E. T. Newton will deliver a lecture on the Reptiles of the Triassic Sandstones of Elgin before the Geologists' Association at their next meeting on May 5. The remains, or restorations, of these extraordinary animals will be shown by the oxy-hydrogen lantern.

Mr. Robert T. Hill, in the *American Journal of Science* for April, gives a description of the Cretaceous formations of Mexico. The geology of Mexico is so imperfectly known that this contribution will be welcome. We observe that the vast mass of blue or grey limestone, known in Mexico as "Mountain Limestone," which forms so important a constituent of the Sierras, is now considered to be entirely of Cretaceous age. True Palæozoic limestones are almost entirely absent from Mexico.

In the April *Scottish Geographical Magazine* will be found an interesting account of the Nile Valley, its physical geography and geology. The outline is particularly valuable, being written by Colonel Justin C. Ross, who, for several years, was Inspector-General of Irrigation in Egypt, and has had occasion thoroughly to study the subject of Nile deposits and floods. The want of a contoured map, unfortunately makes it somewhat difficult to follow the details as to the areas liable to floods, those in which salt tends to accumulate in the soil, etc. Nothing has recently been done to test the depth of the ancient deposits of the Nile, the boring to a depth of 345 feet at Zagazig, made in 1888, being the latest recorded.

MR. MALCOLM LAURIE deals in the current part (vol. xxxvii., pt. i.) of the Edinburgh Royal Society's *Transactions* with some new forms of Eurypterid remains from the Upper Silurian Rocks of the Pentland Hills. So rich was a particular seam of fine-grained sandstone in these extinct "Sea Scorpions," that no less than five new species, including a new genus (*Drepanopterus*), were found. An interesting feature of these species was the large size of their eyes, and the author hints at a possible deep-sea habitat. It is known that many deep-sea creatures, particularly among Crustaceans, are endowed with abnormally large eyes, "as if to make the utmost possible use of the failing light," which (according to one naturalist, at least) does reach even the deepest abysses of the ocean.

THE Transactions of the London Meeting of the International Congress of Hygiene and Demography, held in 1891, have just been issued in the shape of thirteen volumes. These include numerous papers on Bacteriology and other subjects of interest to students of Natural Science.

IN a letter to M. Daubrée, recently read to the Academy of Sciences, Baron Nordenskjöld has called attention to the wide distribution of certain metals hitherto regarded as rare. His analyses of the ash of an anthracitic substance occurring in large nodules in the old rocks near the iron mines of Norberg and Dannemora, in Sweden, showed the presence of the oxides of nickel and uranium (the latter to the extent of 3 per cent.), and the rare earths of cerite and gadolinite.

THE committee of the Alpine Club are taking steps, at the suggestion of one of their members, Captain Marshall Hall, to collect information from colonies and other countries as to glacier motion and history. Such of our readers as are about to visit glacial districts can much assist. Photographs are especially desirable, of shrinkage or increase, and of other changes, and evidence as to any part taken by ice-action in lake formation. They will find an article upon rough surveying and cognate subjects in the *Alpine Journal* for February, 1891, written by the above-named gentleman, who has consented to deal with the materials which it is hoped will result from the action of the committee.

THE Geologists' Association has planned to visit Bath at Whitsuntide. Excursions are arranged to see the Fuller's Earth works at Midford; the Forest Marble, Bradford Clay, and Great Oolite, at Bradford-on-Avon; and the Corallian Beds and Iron works at Westbury, in Wiltshire. It is also expected that one day will be devoted to the classic region of Dundry Hill.

I.

Natural Selection and Lamarckism.

MR. HERBERT SPENCER still believes in the Lamarckian factor in evolution as strongly as ever. In the pages of the *Contemporary Review* he claims such an "Inadequacy of Natural Selection" as necessitates the introduction of the inheritance of the effects of use and disuse as the only means of accounting for the facts he brings forward. I propose to examine his latest arguments, and to show that they are too weak to bear the superstructure which he erects upon them.

The first proof of use-inheritance[1] that Mr. Spencer advances is the fact of the striking differences in the acuteness of the sense of touch in various parts of the body as estimated by the varying distances at which the points of a pair of compasses can be distinguished as yielding separate and distinct sensations. He urges that these differences could not be brought about by Natural Selection, or survival of the fittest, and, therefore, must be due to the cumulative inheritance of the functional modifications produced by use and disuse in the individual. It is admitted, however, that a widely-diffused sense of touch would be evolved by Natural Selection, as being absolutely necessary for safety and continued existence; and it should be equally obvious that Natural Selection cannot act in an indiscriminately unvarying degree in all parts alike, but would evolve local sensibilities solely in proportion to the varying degrees of the usefulness which causes its action. Mr. Spencer himself admits that the high perceptive power possessed by the end of the forefinger "may" have arisen by survival of the fittest; and there ought to be no difficulty in extending the concession to the other finger-tips, and to other joints of the fingers and parts of the hand where it would prove useful, and where its partial diffusion would be aided by the general principle of correlation. The general distribution of the sense of touch is in accordance with the requirements of Natural Selection.

[1] I employ this term to signify "the inheritance of the effects of use and disuse." Besides being brief and convenient, it also has the advantage of not necessarily including the inheritance of such acquired characters as mutilations—a subject on which the more prudent Lamarckians appear to be rather sceptical. One of them, indeed, has distinctly denied the inheritance of mutilations and injuries.

Tactile sensibility scarcely exists in the internal organs, where it would be worse than useless. It is most delicate in the tip of the tongue, which keenly examines food by touch as well as by taste, and in the tips of the fingers, which serve as tactile and manipulative organs. The face, though including highly important organs, does not need so discriminative a perceptiveness as the tongue and finger, and accordingly possesses much less even in the lips and the tip of the nose, where its tactile sensibility is finest. The body and limbs, which have never needed a highly discriminative sense of touch, have never evolved such keen sensibility, so far as is known, and the Neo-Darwinian has, therefore, no need to adopt the suggestion of degeneracy under panmixia so superfluously made on his behalf. The back of the head and body has less perceptive power than the front, where it has been more needed, since we move in a forward direction, and meet or touch objects in front of us more often than objects in the rear. Great part of our tactile equipment dates back to very remote eras. Tongue, fingers, toes, lips, and nose must have evolved special tactile powers even before our ancestors had arrived at the Simian stage of evolution; and in passing through foliage, branches, and thorny bushes a fairly developed sense of touch in the face would be far more serviceable than in the back of the head, and the chest and limbs might well become somewhat more discriminative than the back.

While the relative sensibility of parts can in the main be accounted for by the Neo-Darwinian, it must be freely admitted that we cannot fully and decisively explain every detail, every minor modification, and every gradation, by the principle of the survival of the fittest. We may be unable, for instance, to explain why the thigh and fore-arm are less sensitive in the middle than near the end, or why the skin over the malar bone is less discriminative than other parts of the cheek, or why the tip of the nose retains a measure of tactile perceptiveness exceeding that of the palm of the hand; but our failure to unravel and clearly demonstrate every obscure cause of the minor facts of variation and evolution proves nothing. Even if we accept Mr. Spencer's view of the inadequacy of Neo-Darwinian factors, we still have to inquire whether use-inheritance will fill the gap, or whether it is equally or still more inadequate to afford the desired explanation. Does it help us to account for the fact that the tip of the nose is four-and-a-half times as discriminative as the back of the hand?[2] Is it reasonable to suppose that the nose comes into contact with objects far more frequently than the back of the hand does? Does it touch things just about as often as the third or lowest joints of the fingers, three times as often as the lower part of the

[2] The numbers in these comparisons might fairly be squared so as to represent the relative number of separate tactile areas in a given space instead of linear distances. Thus estimated, the tip of the nose is *twenty* times as sensitive as the back of the hand.

forehead, and nearly twice as often as the palm of the hand?[3] Is it probable that the palm of the hand touches objects so much less frequently than the immediately adjacent finger joints as to account for its possessing little more than half their perceptiveness? Is the cheek touched twice as often or twice as impressively as the forehead, and nearly three times as often as the back of the hand? Mr. Spencer's hypothesis does not seem to offer any assistance in explaining the facts. While it can be reconciled with them in some respects, it is in direct conflict with them in others. All it can do, indeed, is partly to explain such facts as are already explicable by the natural selection of useful tactile powers. Nothing is gained by the introduction of the Lamarckian factor, which, accordingly, still remains as unproven as before.

Mr. Spencer's explanation rests upon the probably correct opinion that tactile sensibility slightly increases with use, and upon the theory that acquired characters are transmitted to posterity. Now, the greatly increased tactile perceptiveness found in various parts evidently depends, as Mr. Spencer fully recognises, on an increased number of tactile areas possessing distinct and separate communications with corresponding sensation tracts or stations in the brain. Mr. Spencer urges, not merely that practice makes perfect, which would be a legitimate conclusion, but that it actually multiplies the lines of communication with distinct endings or areas in the skin and in the brain. Foster, however, whose authority in matters of physiology is at least as great as Mr. Spencer's, tells us that "The improvement by exercise of the sense of touch must be explained, not by an increased development of the terminal organs, not by a growth of new nerve-fibres in the skin, but by a more exact limitation of the sensational areas in the brain" (*Text-Book of Physiology*, pp. 532, 533, third edition). If Foster is right, how could *extreme* perceptiveness be evolved by the inheritance of a slightly-improved limitation or clearer definition of the partially confused and intermingled areas of sensation? How could the transmission of a more perfect working order explain the formation of large numbers of fresh nerve-fibres provided with fresh sensational organs in the brain, and ending in the skin in corresponding tactile areas which, in the end of the forefinger, must be some 900 times, and in the tip of the tongue some 3,600 times, as small and numerous as in an equal area in the back of the neck? It has always been supposed that increased use caused increased size. In this case it is conveniently, though inconsistently, supposed to produce the opposite effect. The more a tactile area is used the smaller it becomes, until these areas are as minute

[3] If we hold that the discriminative sensibility of the tip of the nose is inherited from ancestral lemurs, which habitually applied the nose to objects as an organ of touch as well as of smell, then the long-continued retention of such seldom-exercised perceptiveness shows that the inherited effects of disuse have been so exceedingly slow and slight as to be altogether problematical.

as in the tip of the tongue, where points only one twenty-fourth of an inch apart can be felt as separate impressions. But is it likely that mere use will diminish the size and multiply the number of the separate structures on which distinct sensations depend? Mr. Spencer leads us to infer that such multiplication had taken place in four cases which he tested[4]; but the assumption that increased use converted twelve tactile areas into fourteen or seventeen has somewhat of the same intrinsic improbability as the idea that a man by using his five fingers sufficiently might convert them into six or eight. Though processes of multiplication by splitting or subdivision are perhaps conceivable, Foster's explanation seems much more reasonable, and the supposed value of the whole case as an inferential proof of use-inheritance becomes correspondingly doubtful.

The next example or proof of use-inheritance to which we are referred is the decrease of the human jaw in civilised races. I have already dealt with this case elsewhere,[5] and have attributed the changes more especially to the combined effects of (1), panmixia; (2), sexual selection; and (3), the natural selection of an economy of structure which would promote lightness and agility besides saving some slight amount of nutriment. *Panmixia*, it seems, is a factor which Mr. Spencer had "excluded as impossible." But it is obvious that Natural Selection may favour large and efficient organs of mastication and strong unbreakable jaw-bones among low savages almost destitute of tools and cookery, living at times on the rudest and harshest food, and often falling victims to violence or accident. If Natural Selection has done this—which I presume will not be disputed—then it is perfectly adequate to cause *some* part of the difference between the jaws of savages and of long-civilised races; and it is only saying the same thing in a slightly changed form when we contend that panmixia, in the form of *non*-selection of large teeth and jaws, is one of the causes of the relative smallness of the teeth and jaw in civilised races. How far panmixia alone can reduce size is a matter not easily susceptible of clearly demonstrative proof, but it is at least obvious that the comparative cessation of selection of large strong teeth and jaws leaves the field more open than before for the action of reducing factors such as economy. If it still be said that Neo-Darwinian factors cannot reduce the jaw, we may point to the decisive fact that the other bones of the skull are distinctly lighter in Europeans than in Australians and negroes, although the skull as

[4] As no measurements were made *before* the increased use, the "clear proof" is clearly imperfect, for there is nothing to show that the sensitiveness of the finger-tips in the two blind youths and the two skilled compositors was not entirely congenital. Compositors capable of becoming "skilled," probably started with a sensitive tactile organisation. Weber's twelfth of an inch, too, is obviously only an approximate measurement, and ought not fairly to be made a standard of comparison for more delicate investigations.

[5] *Are the Effects of Use and Disuse Inherited?* ("Nature" Series.)

measured by its brain-capacity has greatly increased in size.[6] Mr. Spencer cannot possibly say that the thinness and lightness of the bones of the skull are due to disuse, or that the Italian skull has been shortened by disuse. Seeing that Neo-Darwinian factors can reduce skull-bones and soften the frowning glabella so conspicuous in Australian skulls, and that they can also absolutely or relatively shorten the whole skull, why cannot they reduce the jaw-bone and alter its shape and dimensions? Are we to suppose that a skull-bone that happens to be moveable is thereby exempted from the action of all evolutional factors except use-inheritance?

As panmixia and economy must be held to have reduced the weight of the mandible as of other skull-bones, so sexual selection must have modified its shape and size by repressing the brute-like muzzle and large prominent teeth seen in the lower and less humanised races. It has thus favoured the relative prominence of the chin, which is quite as marked a feature in the higher races as the reduction of the teeth and of the more purely dental portion of the jaw.[7] The effect of sexual selection—and of panmixia arising from the survival of delicately-constructed females under male protection and civilisation—is, I think, conclusively shown by the fact that the reduction in female European jaws, as compared with Australian female jaws, is about twice as great as in the case of men.

The reducing and disturbing influence of disuse *during lifetime* is also a further cause of decrease which cannot possibly be denied by the Lamarckian. When these various reducing factors are taken into account, what need is there, or indeed what room is there, for the introduction of the Lamarckian factor? The total reduction in the weight of the male European jaw as compared with that of Australian males, does not seem to be more than 17 per cent., and of this more than a third can be accounted for as being due to a much greater loss of teeth together with consequent alveolar absorption. Another third may fairly be attributed to the Neo-Darwinian factors which had

[6] Excluding the lower jaw, I found that the skulls of European females at the College of Surgeons (mostly Italian) were 12 per cent. lighter than those of Australian females, although the capacity of the cranium had increased by 9 per cent. The weight of male skulls was only reduced by nearly half as much.

[7] The reduction of the teeth appears to be greater than that of the jaw. Dr. Macalister found that the area of the crowns of teeth in Englishmen was over 16 per cent. less than in Australian males (*Nature*, Aug. 18, 1892, p. 380). This, in unworn teeth, would correspond with a reduction of about 23 per cent. in cubic measurement or weight—a much greater decrease than had occurred in the jaws that I weighed. A greater reduction of the teeth is exactly the opposite of what *should* have occurred if the Lamarckian explanation were the correct one—for as the teeth emerge from the gum already formed, and are but little, if at all, susceptible of alteration by subsequent use or disuse, they would either not be modified at all by use-inheritance, or at least would be modified much more slowly than the jaw—a circumstance recognised by Darwin, and often insisted on by Neo-Lamarckians, who say that the relatively rapid reduction of the jaw causes a frequent overcrowding of the teeth.

lightened the rest of the skull by over 5 per cent. in the fifty specimens (mainly Italian) which I weighed. Whatever estimate may be formed of the remaining portion of the reduction in the European jaw, we still have to deduct the effect of disuse during lifetime and the direct or transferred effect of sexual and social selection in repressing prognathism. What grounds can we have for assuming that any residue remains which can justly be claimed as a proof of the inheritance of the effects of ancestral disuse? [8]

In endeavouring to establish the inadequacy of Natural Selection, Mr. Spencer puts forth contentions or assertions which manifestly overpass the bounds of accurate and logical reasoning. He holds, for instance, that if Natural Selection does not cause "a *frequent* survival" of individuals possessing any particular trait, it can "neither cause nor aid" any change in that direction. But surely it is as rigidly inevitable as any arithmetical or mathematical fact that the *occasional* elimination of an unfavourable characteristic *must* proportionally affect the general average. Nay, the elimination of a single individual thus characterised has, *ipso facto*, altered the average among the survivors. There is not the least reason, therefore, for accepting the strangely unphilosophical dictum that an evolutional cause or factor will not act at all unless it acts in considerable or important degrees. We may safely allow science and reason to implant in us a perfectly firm and unwavering conviction that every cause, however weak, will produce its proportional effect; and we may expect that the long-continued and cumulative effect of even infinitesimal forces or tendencies may become perceptible in the course of sufficient time.

[8] Mr. Spencer has unintentionally exaggerated the extent of the diminution. A comparison of his statements (*Principles of Biology*, § 166, footnote) with the skulls which he inspected at the College of Surgeons will prove to any careful measurer that the jaw, which was too hastily assumed to be "an average English jaw," must actually have been the smallest English jaw in the collection—that, namely of a female skeleton (No. 70A) with exceptionally delicate features. Founded on such a basis, his ideas and statements concerning the "dwindling away" of the jaw go far beyond the sober facts. As Mr. Spencer's disciples thus learn to overrate the decrease of the jaw, so also they become apt to underrate the reducing causes. Thus Mr. F. H. Collins (compiler of "An Epitome of the Synthetic Philosophy," an abstract of Mr. Spencer's works and teachings, published with an approving preface from the hand of the master himself) represents the amount of nutriment required by the whole jaw as a grain a day and calls this "a large estimate" (*The Diminution of the Jaw in the Civilised Races an Effect of Disuse*, p. 12). A little arithmetic will show that the proportional share of daily nutriment for the jaws with their muscles, &c., is some sixty times greater than this, without including liquid. Master and pupil alike seriously underrate the extent of variation, and ignore the facts that the reduction in the jaw is largely correlated with a similar reduction in the skull as a whole, and that the reduction of weight to be moved would allow a proportional economy in the bones and muscles and limbs and body that carry the lightened head with its lightened jaw; and all this allows still further and complicatedly cumulative economy in the alimentary, circulatory, respiratory, and food-procuring organs, which only have to provide a lessened amount of duly prepared nutriment for the economised parts. Arithmetically estimated, the case for economy is made to appear hundreds of times weaker than a fair consideration of the relevant facts would justify.

Whatever may be said to the contrary, the "widely accepted" view that "Natural Selection will [tend to] increase *any* advantageous trait" is a sound one; for the very meaning of "advantageous" is that some advantage is conferred, that the welfare of the organism is aided, and thereby its survival and multiplication. Of course, the degree of power or influence of Natural Selection will strictly depend upon the degree of advantage, and in proportion as this is slight the corresponding effect will be slight, and may easily be counteracted or masked by the effects of other causes.

This idea that survival of the fittest can do nothing at all unless it is "frequent," is allied with further misconceptions of the way in which Natural Selection brings about evolutional changes. Mr. Spencer compares each generation with the preceding one and finds that the change is too slight to affect survival. I venture to insist, however, and with all possible emphasis, that the basis of comparison of advantage must *not* be the trivial amount of change occurring in each successive generation, but the much more notable variations seen in contemporary individuals. If individuals who are the most poorly adapted in any respect are occasionally cut off, while the more perfect are preserved, there may well ensue some amount of evolutional change, for the average may thus be altered, although the intermediate or mediocre majority were not appreciably affected by the eliminative and selective process. The differences or variations that appear are far greater than Mr. Spencer would allow us to suppose. Thus, some English jaws are two-and-a-half times as heavy as others, while he only concedes a possible variation of about 10 per cent. for Natural Selection to work upon, and makes this allowance, moreover, appear preposterously beyond the degree of variation which would actually be presented to the operation of selective factors. Such fanciful presentments of cases misrepresent the problem before us, and blind us to the actual methods of Nature, whose selective factors naturally act more decisively upon extremes than upon average mediocrity and the minuter degrees of variation. Those who think that Natural Selection acts only upon the almost imperceptible change in each generation, and that this change is too slight to be thus acted upon, ought logically to give up Natural Selection altogether. If the argument were valid, it is hard to see how any of our organs could have been evolved or influenced by Natural Selection. Even the evolution of our most rapidly-evolving organ, the brain, could not be thus brought about, for such evolution has been exceedingly slow, and the advance in each generation could be represented as much too insignificant to affect survival, besides being neutralised by the advantages of superiorities in other organs, as a Lamarckian might also urge.

Another curious limitation imposed on the potency of Natural Selection is its alleged inadequacy to bring about complex evolution. It is acknowledged that it can "keep all faculties up to the mark"

by destroying such individuals as have faculties in some respects below the mark. Natural Selection is thus credited with an all-round elevating power, for it raises all faculties to a higher level than they would otherwise have fallen to. Why, then, does its power abruptly cease at a certain line? If it can thus lift all faculties up to the mark under ordinary conditions, why cannot it raise them slightly above the mark if those conditions become more stringent? If it previously counteracted a general retreat all along the line, why cannot it now cause a general advance? Will an increase of eliminative and selective rigour produce no extra effect?[9] And if it raises aggregates of faculties, must it not raise the separate faculties of which the aggregates are composed? The complex evolution of many co-ordinated organs simultaneously is, of course, a much slower and more difficult process than the special evolution of a single organ or faculty which becomes exceptionally useful; but difficulty is not impossibility, and we are, therefore, fully at liberty to believe, with Darwin, that Natural Selection alone would suffice to bring about the many co-ordinated changes necessitated by the gradual development of the huge horns of the Irish elk or the long neck of the giraffe. As a matter of fact, complex evolution of special organs and complicated instincts *has* taken place without the help of use-inheritance, and in spite of the strongest opposition it can offer, as is seen in neuter insects, such as bees, and still more in some species of ants, where workers, soldiers, and another distinct caste, apparently of overseers, are descended from innumerable generations of helpless parents who do not develop various organs and instincts which the neuters alone can improve by exercise but cannot possibly transmit to posterity. To hold that Natural Selection is totally incapable of bringing about complex evolution is to hold that the evolution of bees and ants is impossible. Seeing, then, that Natural Selection has evolved the (relatively) huge head and fighting mandibles of the soldier-ant,[10] with all the complex co-ordinated changes involved, there is no reason for supposing that it cannot have effected similarly complicated changes in larger animals, such as the elk and the giraffe.

The influence of beneficial modifications in determining survival is not easily measured. Advantages may often be more decisive than we are apt to suppose. Thus the discriminative tactile sensibility of the tip of the tongue seems to have been of some importance in promoting welfare and survival. It greatly aids the sense of taste in securing fit and proper nutrition—a consideration entirely overlooked by Mr. Spencer. While constantly exploring small irregularities of

[9] This rigour might give itself fuller scope by first increasing fertility as a point of supreme importance or absolute necessity.

[10] Among termites, too, the soldier caste possesses "enormously large, hard, and strong heads, almost as big as the rest of their bodies, and these are armed with gigantic and very strong and sharp mandibles, while the heads and mandibles of the non-combatant workers are much smaller and weaker."

surface, it is minutely ascertaining the texture, so to speak, of the organic or inorganic substances submitted to the ordeal of its keen judgment, and by the general structure and softness of the particles or fibres it determines whether they shall be accepted as digestible food or avoided as injurious or innutritious. It detects and rejects sand, earth, grass, leaves, or other unpromising material with which men from mere laziness, or the pressure of famine, would otherwise gratify their hunger, to the injury of the whole system.[11] It discovers and enables us to remove such objects as fish-bones, hair, feathers, and pieces of grit (which latter, as I have personally experienced, may splinter a tooth) and so saves internal organs from injury by their accumulation or friction, and from waste of digestive activities. The tongue also delicately manipulates the food, and secures its proper mastication by pushing it between the teeth, while carefully avoiding being bitten between the little grinding mills and cutting machines amid which it works so deftly and yet so safely. It is the highly and wonderfully discriminative guardian and janitor of the whole alimentary canal, and the supreme judge of the quality, composition, and admissibility of the nutriment which is the foremost essential of life. If Natural Selection cannot evolve a very special degree of sensitiveness in the tongue, it is hard to see what it can evolve. Mr. Spencer, however, contends that "no advantage is gained" by the explorations of the tongue, that the extreme perceptiveness of its tip is not needed, that use-inheritance furnishes the only possible explanation, and that even if tactile sensibility is of use in detecting fish-bones and foreign bodies among the food, a degree of sensitiveness equal to that of the finger-tip (which is only half as sensitive as the tongue-tip) would have been sufficient. But who can fairly undertake to decide that the higher degree of sensitiveness in an important food-selecting organ would never promote the well-being of individuals so far as to lead to survival where others perished? If, moreover, lower powers occasionally led to elimination while surplus powers did not, there might easily be an evolution of surplus powers—a principle which will help to explain many of our keener and finer powers of eye and ear and brain, and the surplus powers of organs generally.

In all cases of minor advantages, especially those of economy and lightness, it must constantly be borne in mind that the survival test acts more specially and most strongly in occasional crises—such as those of illness, famine, war, accident, and other perils—through some of which all of us still have to pass from infancy onwards. In such cases, exceedingly minute advantages may sometimes turn the scales of life and death. Even now, although the rigour of Natural Selection is so greatly mitigated under civilisation, a slightly healthier, stronger, and better nourished body, due to lessened waste

[11] Some tribes, when hungry or starving, partly evade or deceive this tactile sensitiveness and repugnance by filling their empty stomachs with a fine clay, or a fine, flour-like powder composed of exceedingly minute shells.

of food or tissue, or to other slight advantages, may carry child or man through fevers otherwise fatal, or through physical dangers and hardships, or arduous labours and mental trials under which he would otherwise have succumbed. In the not very remote past, a lighter skull, by diminishing the weight to be moved, and so increasing a man's speed and agility, may have enabled him to save his life by flight from enemies, or to overtake and kill a less swift and agile foe. Considering correlated changes and advantages, and looking at the many and varied tests through which our species has had to pass, at the critical episodes of personal strife, the occasional narrow escapes from natural dangers, and the innumerable diseases, parasites, microbes, &c., that waylay us through life, and search out the weaker points, it becomes almost impossible to say that any advantage has been so minute as never to have affected survival. Under starvation, wounds, disasters, illnesses, men hover on the brink of the grave, and a hair's-breadth of advantage, a single grain of spare nutriment or lessened expenditure, may decide ultimate recovery, while any little disadvantage may prove the last straw that breaks the camel's back. All organisms pass through kindred dangers and diseases of which we know little and care less. As a rule, the lower animals suffer far more frequently and severely from starvation and enemies than man does; they multiply much faster, and they are eliminated with proportional frequency. In view of such facts, we are by no means bound to suppose that small economies can never influence life or death in their immediate or ultimate effects.

Darwin did not disdain to call in the effects of slight causes. He partly attributes the somewhat reduced size of our teeth to the principle of economy,[12] although a tooth is scarcely $\frac{1}{300000}$th part of the weight of a man's body. Mr. Spencer, however, thinks that we ought to feel certain that economy, panmixia, and reversed selection, all put together, cannot reduce the useless, exposed, and sensitive eyes of the proteus and other "blind fish and amphibia" living in dark caves. We are told, indeed, that "an economy of $\frac{1}{2000}$th of the creature's weight could not appreciably affect survival" (p. 166).[13] But if Natural Selection were really incompetent to secure economy in small parts, it could not secure economy in the body as a whole.

[12] *Descent of Man*, p. 562.

[13] Such an economy is equivalent to the saving of three-quarters of a pound in man. Many circumstances are conceivable in which death might be avoided and multiplication be favoured by slightly increased facility and quickness of motion, by surplus nutriment, and lessened liability to various dangers. Disused parts retain, or, apparently even increase, their share of liability to tumours, ulcers, and other diseased conditions which occasionally prove fatal by their direct or indirect results—of which many examples are known in man, the only animal with whose pathology we are really well acquainted. Probably, too, as Ray Lankester points out, there was a preliminary selection of animals whose imperfect eyes led them into the sheltering darkness of the cave, and prevented them from straying from it.

If no saving can be effected in the small separate fibres that make up muscle and nerve—if economy cannot be brought about in the separate cubic inches, or ounces, or grammes, of the bodily structure, how can it possibly be effected in the whole body, made up of those separate inches, or ounces, or grammes? Mr. Spencer allows that Natural Selection, and, therefore, panmixia also, can act upon aggregates; and he is, therefore, logically bound to admit that they can affect the minute parts of which aggregates are composed. The changes are not necessarily as gradual as Mr. Spencer represents them. Greatly diminished, or almost aborted, eyes will occasionally appear, and the possessors, no longer eliminated through failure of vision, will diffuse the degenerative tendency among the species by intercrossing.

Fortunately, the question of the possibility of minute reduction and minute economy in the shape and weight of organs, without the aid of use-inheritance, can be settled by a simple and direct appeal to facts. The advice of Solomon, that we should go to the ant to learn wisdom, is peculiarly applicable to the Neo-Lamarckian. He can test his arguments by an appeal to Nature herself. In neuter insects we are furnished with a happy opportunity of excluding use-inheritance from the problem. Now, in neuter insects, the effects assumed to be impossible have actually occurred. Neuter ants of various species possess eyes in all manner of grades of imperfection and smallness down to the most rudimentary, and even to the complete loss of the eye, of which only a trace of the socket remains. This, like the loss of wings, cannot be due to the inheritance of acquired characters, since the neuters cannot transmit their acquired characters to posterity, and the actual parents still retain the eyes and wings necessary for the nuptial flight. What, then, is the use of elaborately arguing that accomplished facts are impossible? It is the ways and works of Nature that we must study. She cares nothing for the theories of philosophers and their fancied impossibilities. She calmly *does* the thing declared impossible; in neuter insects she reduces and economises on the minutest scale without use-inheritance; she neatly and fitly develops, with all due co-ordination and economy, the special organs of neuter insects, together with wonderful mental faculties, and complex social instincts, which are never exercised, or, indeed, possessed, by the parents. Why, then, should we be asked to believe that, without the help of use-inheritance, minute economy and complex evolution are incredible? And what argument can possibly show that processes which certainly take place in innumerable species of ants, bees, termites, and wasps cannot take place in larger organisms where we do not happen to possess any special means of demonstratively separating the effects of Neo-Darwinian factors from those attributed to the transmission of acquired characters?

With reference to the arguments advanced against Weismann, I may say briefly that, although the bodily, or somatic, elements which

shelter and nourish the reproductive elements can produce some kind of effect upon them, and though reproductive elements may persistently affect other reproductive elements, as in the case of Lord Morton's mare, yet such and similar facts do not in the least decide the question whether the cells, or units, of the bodily structure can affect the reproductive substance, or germ-plasm, in such a way as to convey their particular acquired modifications, or changes, to posterity.

A few words are also necessary in answer to the claim that since congenital characters are heritable, the *onus* of proving that acquired characters are "not inheritable" falls upon the Neo-Darwinian. The implication that the one case affects the other cannot be sustained. It is perfectly certain and perfectly well-known that there is a wide difference between the two classes of characters, and that they are not comparable in their degree of effect upon heredity—otherwise there would be no more dispute concerning the one class than concerning the other. No one questions the immediate and decisive heritability of *congenital* mutilations and malformations, or of any other congenital peculiarity, however striking or minute. On the other hand, it is a matter of general experience that the loss of a tooth, or eye, or limb, or finger-joint, is no more transmitted to offspring than a sunburnt complexion, or a clipped beard, or a knowledge of Latin or cookery. In proportion as the nature of each case admits of clear decisive proof, we see that acquired characters are *not* inherited; no indisputable evidence to the contrary has ever been given. The presumption arising from the *proven* facts, therefore, is that non-inheritance of acquired characters is the general rule, and the *onus probandi*, therefore, rests with those who assert the contrary. We cannot prove a universal negative; and it is not easy to find or suggest absolutely decisive tests of the admittedly slight power of use-inheritance, or of the size-reducing power of panmixia. The effect of use-inheritance on offspring is imperceptible. Darwin acknowledges that several generations must elapse before any appreciable result follows;[14] and during this interval other factors would be at work. We perceive the apparently insuperable difficulty, or practical impossibility, of excluding all other factors in such delicate and complicated cases. But if the Neo-Lamarckian will accept his own cases, and allow us to adopt his own assumption that extraneous factors are adequately excluded, we can show that the effects of use and disuse are *not* inherited in some features just as conclusively (or inconclusively) as he shows that, in other respects, they are inherited. If, for instance, use-inheritance is proved by the shortening of the less used wing-bones of the domestic duck, or the leg-bones of the rabbit, then it is disproved by the thickening of these bones; and, conversely, if it is proved by the marked thinness or lightness of the wing-bones of the

[14] *Variation of Animals and Plants under Domestication*, vol. ii., p. 288.

domestic fowl, it is also disproved by a non-shortening, or extremely slight shortening, which is most conspicuous in the breeds that never fly. But the Neo-Lamarckian will probably spike his own guns rather than allow us to use them against him ; and we must allow him to do so, if he chooses. The one thing clearly provable is the excessive dubiousness of a factor which is so strangely uncertain in its action, or is so weak as to be easily overruled in some of its effects by causes so trivial and obscure that their interference was never suspected.

We have seen that the theory of use-inheritance is unnecessary, since Natural Selection and other non-Lamarckian factors are evidently capable of effecting such changes as have been adduced. When we further consider the many difficulties and improbabilities involved in the hypothesis—the suspicious lack of satisfactory evidence—the evidence to the contrary which can be drawn even from an examination of the details of the very cases presented as proofs—the ease with which the effects of Natural Selection and panmixia are attributed to use or disuse—the certainty that mutilations are not inherited even when (as in the case of the hymen) they have been repeated for innumerable generations—and the extreme difficulty of accounting for transmission of acquired characters except by the overwhelmingly cumbrous and improbable method of pangenesis—we may well come to the conclusion that the inheritance of acquired characters cannot safely be admitted as a factor of evolution unless much better evidence is produced than has hitherto been forthcoming ; and until such proof of the Lamarckian factor is obtained, we can have no right to rely upon a mere hypothesis which biologists are fast rejecting as a mere figment of the human imagination. And if the growing opinion of scientific men is correct, Mr. Spencer's psychology and many cherished but illusive expectations of continued progress by education alone will prove to be but castles built on sand, and will have to yield due supremacy to selective ideals and methods corresponding with the fundamental laws of evolution and of mental and moral progress —a position which by no means excludes the beneficent effects of education and parental influence, although it rejects the delusive and mischievous belief in the alleged inheritance of the changes thus produced.

Wm. Platt Ball.

II.

Biological Theories.

IV.—SUPPOSED AUDITORY ORGANS.

THREE groups of phenomena appear to have been imperfectly distinguished in the minds of those who have ascribed auditory functions to the organs known as tentaculocysts in jelly-fishes, as otocysts in mollusca and some crustacea (*e.g.*, *Mysis*), as auditory sacs in lobsters, crayfishes, etc., and as auditory hairs or setæ in the same and other crustaceans.

The groups of phenomena are those associated with the transmission of sound-waves in air, of sound-waves in water, and of waves of other kinds in water. The nature of the confusion will be seen by a consideration of the following quotation from Sir John Lubbock's "Senses of Animals":—

"Hensen took a *Mysis*, and fixed it in such a position that he could watch particular hairs with a microscope. He then sounded a scale: to most of the notes the hair remained entirely passive, but to some one it responded so violently and vibrated so rapidly as to become invisible." "*That these plumose hairs then really serve for hearing may be inferred . . . . . from the observed fact that they respond to sound-vibrations.*" "Hensen's observations have been repeated and verified by Helmholtz." (*Op. cit.*, pp. 93, 94).

The portions of the passage which I have omitted refer to the existence of nerves in relation with these hairs.

Two criticisms are at once suggested. In the first place, the inference is unsound. Because the human eyelashes "respond to sound-vibrations," it is not safe to infer that they "really serve for hearing," and it is not any more safe to infer from the "observed fact" that certain plumose hairs in Mysis "respond to sound-vibrations" that these serve for hearing.

In the second place, there appears to have been a misapprehension as to what constitutes a sound-wave in water.

If a sounding tuning-fork be pressed against the side of an aquarium, two kinds of waves are produced in the water, surface-waves and sound-waves. The surface-waves are visible to the naked eye and travel slowly across the surface, that is, sufficiently slowly to be followed by the eye. Each of these is a *movement* of the water,

partly up-and-down but partly in other directions. The details of this wave-movement will have to be considered in a future article, and it is only necessary here to recognise that the wave consists of an obvious movement; that its velocity is small enough to enable the progress of the wave to be watched; that it is transmitted only through a superficial layer of the water, the thickness of which varies with the intensity of the disturbance; and, finally, that its propagation depends upon gravitation and surface-tension.

The sound-waves agree in number with the surface-waves, and there the agreement practically ends. These waves are waves of compression rather than of movement; their rate of transmission is about a mile a second; their propagation depends entirely upon the elasticity of the water, that is, the property by virtue of which the water resists voluminal compression, and regains its original volume after compression; the movement of the water involved in the transmission of such waves, even when the sound is of explosive intensity, is far too minute to be seen; this minute movement is, moreover, strictly to and fro in the direction of propagation of the wave; the wave is transmitted not through a superficial layer only, but in all directions through the water.

If the aquarium be divided into two parts by a water-tight partition of wood, glass, iron, or other solid, the surface-waves will be stopped by this partition, while the sound-waves will pass unimpeded through it.

Sound-waves in air differ from those in water in the amount of movement of the particles transmitting the wave. Here, also, the wave is one of pressure rather than of movement, though movement ten thousand times as great as that in an aquatic sound-wave is involved.

Some idea of the minuteness of these movements may be obtained from the determination made by Lord Rayleigh and recorded in Ganot's "Physics." The note experimented upon was f^{IV} produced by an organ pipe. The intensity was such that the note was audible at a distance of more than half-a-mile. The movement of each air-particle was less than one two-hundred-and-fifty-millionth of an inch, *i.e.*, little more than one five-hundred-millionth of a wave-length.

Water is only one ten-thousandth part so compressible as air, and a like variation in pressure would, therefore, only involve in water a movement of each particle of about one five-billionth of a wave-length.

Whether Hensen's experiments were performed in a vessel sufficiently large for the accommodation of a series of waves each five billion times the length of the observed movements of the hairs or not, I cannot say,—the Atlantic Ocean would be a vessel of dimensions suitable for the experiment. What the notes were to which the hairs "responded" I do not know—suppose, however, the lowest

note which is audible to man to have been one of them—*i.e.*, a note produced by sixteen vibrations per second, or thereabouts. In water this note has a wave-length of half-a-furlong, *i.e.*, nearly four thousand inches. One five-billionth of this is one one-thousand-two-hundred-and-fifty-millionth of an inch ($\frac{1}{1250000000}$ inch). This can hardly be the length of the "violent" vibration which is said to have rendered the hairs invisible. Suppose the language used in describing the result to be not greatly exaggerated, we may safely say that a movement at least one million times greater than this was referred to. This is, however, the greatest movement producible in water by the passage of any audible note when the intensity is such as to render the note just audible at a distance of half-a-mile. The employment of notes of this low pitch, and of higher intensity than this, would be fraught with great danger, for the apparatus of the middle ear of man could hardly stand much more. To reach the intensity required for the production of the above result, a steam "devil" such as is used on large steamers would be required, and would have to be close to the ear of the observer who was watching the hairs under the microscope. The man who would risk putting his ear within a foot or two of such a source of sound when this was put into operation to the utmost limits of its powers would indeed be a bold man. I doubt, however, if the most powerful of these "whistles" ever made has ever been heard at a distance of thirty miles. Supposing $\frac{1}{1250}$ of an inch to be the extent of the violent movement rendering the hairs invisible, *i.e.*, a little over twice the diameter of a red blood-corpuscle, then the intensity of the sound required, supposing the note to be C_{IV} (=16 vibrations per second), would be such as to render it audible at a distance of 500 miles in air, and much further in water. Supposing it to be at such distance from the head of the observer as to have an intensity equal (in air) to that operating (in water) on the hairs, then the air in contact with his head would move with an amplitude of about eight inches, and with a force which no human skull could withstand for a moment. A microscope would be instantly destroyed by it. I doubt whether even any ordinary building could resist its destructive power, and do not for a moment believe it possible to construct an instrument capable of "sounding a scale" with such intensity as this.

We may, therefore, in spite of the authority of so eminent a physicist as Helmholtz, rest perfectly assured that the effects described have never been either produced or repeated or verified by either Hensen or anybody else. It is, however, not the record of the experimental results which is at fault, but the explanation of them. Vibrations in the water produced by the sound-waves in air, or by vibrations communicated from the source of sound through the table, microscope, etc., have been mistaken for sound-waves in water.

It is, perhaps, inaccurate to assume, as I have done, that the

intensity of a sound (whatever its pitch) is proportional to the range of pressure through which the transmitting medium passes during the transmission of the wave. Making allowance, however, for this error, the result would be the same. The intensity of wave, in water, necessary to produce a vibration of one-thousandth of an inch would in air produce a movement of about ten inches, and would involve changes of pressure which the human skull could certainly not resist, whether the walls of the building could or could not.

"Otocyst" is a name given to a sac, usually microscopic, lined with a sensory epithelium, usually, if not always, provided with sensory hairs. The sac encloses an "otolith," that is, a mass of some substance (usually calcareous) denser than the surrounding water or the tissues of the otocyst. The otolith may be replaced by several masses of some dense solid.

These, as the name "otocyst" signifies, are regarded, if not by all, at least by most zoologists, as auditory organs, and their action is said to depend upon the jolting of the hairs against the otolith or (which is worse!) *vice versâ*.

Perhaps what has been written above may convince zoologists that the production of any such jolting by an *aquatic* sound-wave is a physical impossibility. It is barely conceivable that any wave should involve pressures so great as to reduce the volume of any mass of substance to absolutely nothing, that is, to cause the matter of which it is composed to cease to exist. No pressure short of this would, however, bring the otolith into contact with the sensory hairs in question, even if they were, to begin with, at an infinitesimal distance. The fluid pressures we have to do with act equally in all directions. No movement, except in the direction of the wave-propagation, is produced: there can be no flowing outwards to the sides of even an infinitesimal amount of water which was between the tip of the hair and the otolith, for the pressure at the sides is equal to that between the hair-tip and the otolith.

Whatever the function may be of such organs as otocysts, tentaculocysts, auditory sacs, auditory hairs of marine crustacea, "ears" of fishes devoid of air-bladders, or, in fact, of any organ whatever, in any aquatic animal, that these organs, unless they be associated with a cavity containing a gas, should serve as auditory organs is simply a physical impossibility. An auditory organ would, moreover, be useless to such an animal as a jelly-fish, and the evolution of such organs in such animals could not be explained by Natural Selection. A real function of some of these organs will be treated of in a future article.

C. Herbert Hurst.

III.

The Fruit-Spike of Calamites:

A CHAPTER FROM THE HISTORY OF FOSSIL BOTANY.

FOR many years there has been much doubt and uncertainty among palæobotanists as to whether or not the fossil spike now known as *Calamostachys Binneyana*, Schimp., is entitled to be recognised as the fruit of *Calamites*. It was so regarded by Carruthers (1) in 1867 and by Schimper (2) in 1869, and Binney (3), who described it in 1868 as the fruit of *Calamodendron commune*, was practically of the same opinion, since the plant so named by him is one of the forms of *Calamites*. Williamson (4), on the other hand, has persistently maintained that its affinities are rather with the Lycopodiaceæ, and that the only spike entitled to rank as the fruit of *Calamites* is the one described by him in 1869, and more fully in 1888, and which is very different in many respects from *Calamostachys Binneyana*.

Recently the present writer has had an opportunity of studying a fine series of new preparations of the spike, belonging to Mr. W. Cash, of Halifax, and in a paper shortly to appear in the *Proceedings* of the Yorkshire Geological and Polytechnic Society, he claims to have fully established the fact that it is the spike of some form of *Calamites*. The time seems opportune, therefore, for a brief historical retrospect of the treatment accorded to *Calamostachys Binneyana* by those palæobotanists who have endeavoured to determine its affinities from a study of its structure.

As described by Carruthers in 1867, *Calamostachys Binneyana* consists of an axis, around which are placed in close array a number of whorled appendages. These appendages are of two kinds, whorls of sterile bracts alternating regularly with whorls of stalked structures carrying sporangia containing spores. The latter have peltate heads, and closely resemble the sporangiophores of *Equisetum*—so closely, indeed, that were the alternate whorls of sterile bracts suppressed or replaced by structures of the other type, the spike of *Calamostachys Binneyana* might well be taken for a spike of *Equisetum*.

The specimens at the service of Carruthers appear to have been imperfect and not well preserved, as the minute anatomy of the spike

is only partially given. Among other details, however, it is stated that the axis had "a bundle of fine scalariform tissue in its centre, forming about a third of its diameter, and generally appearing free from the surrounding cellular tissue, which is composed of somewhat elongated cells."

Throughout his description, Carruthers appears to have had no doubt whatever that he was dealing with the fruit of *Calamites*. In this he was probably guided by the great and undoubted similarity of the general characters of the spike to those of *Equisetum*, with which *Calamites* has always been held to have a close relationship. At any rate, he does not seem to have considered that the anatomy of the axis as he then understood it, *i.e.*, with scalariform tissue, and not parenchyma, in the centre, was an insuperable difficulty in the way of a determination supported by so many of the other characters of the spike.

Binney's description of *Calamostachys Binneyana* was published in 1868, and though his account is very unsatisfactory from the botanist's point of view, it implied the presence of a vascular bundle in the centre of the axis, as described by Carruthers. In spite of this, he accepted the view that the affinities of the spike were with *Calamites*, and does not seem to have considered it necessary to explain on this assumption the presence of the axile vascular strand.

In 1871 Williamson began that series of memoirs on "The Organisation of the Fossil Flora of the Coal-measures" which have done so much to increase our knowledge of Carboniferous plants. The first of these deals with *Calamites*, and incidentally the question of its relationship to *Calamostachys Binneyana* is referred to. It is pointed out (5) that in *Calamites* the centre of the stem is occupied by a purely cellular pith with a vascular zone at its periphery, while in *Calamostachys Binneyana* these conditions are reversed. Hence the inference is drawn that if the latter be the fruit-spike of the former, then in passing from one to the other the tissues must undergo a metamorphosis which is without parallel among living plants. In the fifth memoir (1874) the spike is described afresh, and many new details of structure are given with respect to it. Discussing its affinities in the light of the fuller knowledge, the author says (6) :—

"After balancing these various facts and arguments, I am led to the conclusion that *Calamostachys Binneyana* has much closer affinities with *Asterophyllites* than with *Calamites*. With the latter it has no one feature in common. There is no solitary point in which the two plants resemble each other. The resemblance of the fertile sporangia of *Calamostachys* to those of *Equisetum* has been combined with the foregone conclusion that the *Calamites* were Equisetaceous plants, in leading to the belief that the two were parts of the same plant; but I cannot conceive of any conditions in which the stem of a Calamite could be prolonged into that of a *Calamostachys*. I have carefully investigated the relations which the fertile stems of the Equiseta bear to axes of their terminal fruit-spikes, and I find that their respective structures are typically identical. The transition from the stem to

the fruit-axis produces no structural changes save such as are of the most trivial kind; the general type remains unaltered and continuous. But to plant *Calamostachys Binneyana* upon the top of a Calamite would be as abnormal as to surmount the stem of an *Equisetum* with the strobilus of a Lycopod."

Thus, nearly twenty years ago, Williamson took up the position that, notwithstanding the close general similarity between this spike and that of *Equisetum*, the internal anatomy of the axis was so different from that of the stem of *Calamites* that its affinities could not be with the latter plant, but were rather with *Asterophyllites*. At that time he regarded the plant he described as *Asterophyllites* as a somewhat aberrant member of the Lycopodiaceæ (7), so that he virtually placed *Calamostachys Binneyana* in the same group.

In subsequent memoirs he has returned again and again to the affinities of the spike, but always with the object of strengthening the position taken up in the above extract. In the ninth memoir (1878) he expresses (8) surprise that Mr. Carruthers should still continue to believe in its Calamitean character, and points out that "both M. Grand' Eury and Dr. Dawson have fallen into the accidental error" of making him "regard the *Calamostachys Binneyana* as belonging to *Calamites*," whereas he has "most strongly opposed that idea." Two years later (1880) fresh specimens of the spike were described by him (9), with the result that they are said to have "fully confirmed the conclusions" arrived at in 1874, "that *Calamostachys Binneyana* had not the slightest relationship with the *Calamites*, but that it had strong affinities with *Asterophyllites* and *Sphenophyllum*." The following year (1881) is remarkable for the discovery of a new form of *Calamostachys*, which Williamson appears at first to have regarded as identical with *Calamostachys Binneyana*, but which he subsequently named *Cal. Casheana*, after its discoverer, Mr. W. Cash, of Halifax. After a detailed account of its structure, in which great stress is laid on the fact that it contains two kinds of spores, he says (10):—

"It is scarcely necessary to say that this discovery of macrospores and microspores in *Calamostachys Binneyana* supplies another link connecting this strobilus with the Lycopodiaceæ in the same measure that it separates the fruit from the Equisetaceæ. . . . This discovery strengthens my old conviction that the true affinities of this strobilus are with the Lycopodiaceæ."

Finally, in the fourteenth memoir, which appeared in 1888, we have his last pronouncement on the subject, which is as follows (11):—

"After a prolonged conflict the conclusions of those who have insisted upon the cryptogamic character alike of *Calamites* and *Calamodendron*, have met with an extensive, though not universal, acceptance. Meanwhile, both the opposing schools of palæontologists recognise the importance of discovering the fructification of these plants. Mr. Carruthers believed that he had found it in examples of *Calamostachys Binneyana*, and Mr. Binney arrived at a similar conclusion. I have always rejected these conclusions because of the conspicuous differences between the morphology of the Calamitean twig and that of

the axis of the *Calamostachys*. These differences appeared to me too great to make it possible for the one ever to have been a prolongation of the other."

Thus up to his latest utterances on the subject Williamson refused to recognise *Calamostachys Binneyana* as the fruit of *Calamites*, and justly insisted upon the difference between the anatomy of its axis and that of the stem of *Calamites* as being strong evidence against such a view. He even went further than this. So far back as 1874 he maintained (12) "that the only British strobilus of which the internal organisation has hitherto been described, that has any claims to be regarded as the fruit of *Calamites*," is that which he figured and described in the *Transactions* of the Literary and Philosophical Society of Manchester in 1869. Substantially the same statement is made in the memoir of 1888 already referred to, so that on both positive and negative grounds he refused to recognise any connection between *Calamostachys Binneyana* and *Calamites*.

Such was the state of opinion among palæobotanists when the new and much better-preserved specimens already referred to came into the writer's hands. Looked at from the botanist's point of view, it is obvious that the divergence of opinion is based upon the fact that there is a sort of inconsistency, if the expression may be allowed, between the external characters of the spike and the internal anatomy as hitherto understood. As already pointed out, the former have so close a resemblance to those of an *Equisetum* spike, that one naturally looks for the parent plant of *Calamostachys Binneyana* among those fossil forms most nearly allied to *Equisetum*, and by a short step we come to *Calamites*; but, on the other hand, the internal anatomy as described by Carruthers, Binney, and Williamson, is so irreconcilable with the anatomy of the stem of *Calamites* as to justify much that the last authority has written in opposing the Calamitean affinities of the spike.

An examination of the new material gave what the writer thinks is a complete solution of the difficulty thus presented, and at the same time indicated in what direction to look for the reconciliation of these diverse opinions.

In the first place, it soon became evident that the centre of the axis of *Calamostachys Binneyana* was not vascular, as had so frequently been stated, but was composed of cellular tissue which constituted a true pith. In 1891, when drawing up the first part of an Index to his Memoirs, Williamson introduced a brief footnote (13) in connection with *Calamostachys Binneyana*, which shows that he was beginning to have doubts on this point, and he admits that in the specimens described in 1874 "the elongated medullary cells" were "mistaken for tracheids." He says nothing, however, as to whether this parenchyma is a true pith, such as we find in *Calamites*, or merely a central parenchyma like that found in the stele or vascular cylinder of some *Lepidodendra*. On this point, however, the sections referred

to are very explicit, and leave no doubt that the former alternative is the true one.

In the next place, it became clear that the axis differed in a much more important point from the published descriptions of it, and that in a direction which had not been previously suspected. In fact, the specimens proved to demonstration that the cellular pith is surrounded by a ring of primary vascular bundles, which, in essentials, are of the true Calamitean type. The number of these is usually three, though there may be four or more, but, whatever be the number, each is characterised by the presence, at the apex of the xylem, of a carinal canal homologous with the canals found in the same position in the stems of *Calamites* and *Equisetum*, and which represent the tracheal initial strands of the primary vascular bundles. In the transverse sections, we find individual elements clinging to the sides of the canals, and projecting into their cavities, reminding us at once and forcibly of the appearances met with in *Equisetum*. So close is the resemblance, that one might from the transverse sections alone infer that these projecting elements are annular or spiral vessels, but for demonstrative proof recourse must be had to the longitudinal ones. Fortunately, some of these have been made in such favourable directions, and the tissues are so well preserved, that they leave nothing to be desired. They show us the carinal canals cut throughout their whole length, with the annular vessels still attached to the peripheral walls.

Hence it is obvious that the stele or central cylinder of the axis of *Calamostachys Binneyana* has a structure which is identical in essentials with that found in the stem of *Calamites*, and there is no such antagonism between the anatomy of the two as has hitherto been believed. Not only so, but when, as sometimes occurs, the formation of secondary xylem begins, it does so first at the carinal canals, as in *Calamites*, and subsequently in the intervening areas. In this way is brought about the triangular form of the pith, and the triquetrous arrangement of the vascular laminæ which surround it, and which Williamson compared with the secondary xylem of the plant he named *Asterophyllites*.

The conclusion, then, is, that the correspondence between the anatomy of the stem of *Calamites* and the axis of *Calamostachys Binneyana* is so close and definite as to leave no doubt whatever that the one is the fruit-spike of the other. Williamson's objections to this determination have all been based upon the supposed absence of such a correspondence, and now that it is shown to exist they naturally fall to the ground. The internal anatomy of the spike, then, points to the same affinities as the external characters, and together they supply all the evidence that is needed to establish the conclusion we have stated. To which of the forms of *Calamites* the spike is to be more particularly referred cannot at present be definitely stated, but there are some reasons for thinking that it must be sought

among those of the type of *Arthropitys*. This, however, is a matter of secondary importance compared with the fact that, after so many years of controversy and doubt, we have, at last, got clear and conclusive evidence that *Calamostachys Binneyana* is the fruit-spike of some form of *Calamites*.

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THOMAS HICK.

IV.

The Succession of Teeth in Mammals.

THE question as to the mode of origin of the successional teeth of Mammalia is one which has at various times received a considerable amount of attention from anatomists, and one which it may be of some interest to review in the light of recent researches by Dr. Kükenthal.[1]

In many of the Mammalia, as is well known, there are two sets of teeth, the first, or milk teeth, which are usually deciduous, and followed by a second more permanent set. In numerous reptiles and fish, on the other hand, we find a perpetual succession of teeth. Instead of the sets being limited to two, there is a constant replacement; as fast as one series of teeth wears away a new series appears to take its place. Moreover, in these reptiles and fish the replacement is also more complete than in Mammalia, for whereas in the latter we never find the whole series of teeth replaced, the true molars being never preceded or followed by others, in the fish and reptiles, when there is replacement of one there is replacement of all the teeth.

When the contrast between this diphyodont condition of Mammalia and the polyphyodont one of Reptilia is realised, the question most naturally arises, is the diphyodontism of the one a relic of the polyphyodontism of the other, or is it to be regarded as an advance from a monophyodont state, one in which a single set of teeth alone was possessed?

Two years ago much might have been said in favour of a primitive monophyodont condition by an appeal to the simplest Mammalia. In the first place, in the toothed whales, the Mammalia possessed of the most reptilian type of teeth and jaw, only one set of teeth was known. Again, in the Monotremes there is no replacement, and, indeed, the teeth which do exist are rarely used, being generally concealed beneath horny plates. Lastly, in the Marsupials only one tooth in each jaw, the posterior premolar, is ever replaced, and this seemed to give us the starting point for the more complete change occurring in higher forms.

Dr. Kükenthal's recent work has, however, invalidated two of

[1] *Ann. Mag. Nat. Hist.*, 1892.

these arguments, and in April of last year an authority as well qualified to speak on the subject as Mr. Oldfield Thomas, declared in favour of a diphyodont condition being the most primitive one in Mammalia—a condition, moreover, which was probably derived from the polyphyodontism of Reptilia.

The first argument drawn from the Odontocetes, the toothed whales, is shattered by the discovery of an embryonic series of teeth within those which later pierce the gum to persist as the teeth of the adult. In an advanced embryo of a dolphin (*Phocæna communis*), possessed of the twenty-five teeth in each mandible, rudiments of a second series were found internal to these. Kükenthal accordingly pronounces the teeth which are present throughout the life of these whales as representatives of the deciduous teeth of other Mammalia, while the uncut rudiments would correspond with the adult dentition of higher forms. These rudiments are, although small, perfectly recognisable as teeth, and are possessed of a cap of enamel and an inner core of pulp.

It was the Marsupials, however, that supplied the chief arguments to Sir W. H. Flower and Mr. Oldfield Thomas as to the secondary character of the diphyodont condition in Mammalia, and the most important part of Kükenthal's work on this subject is, therefore, the discovery of an almost complete series of successional teeth in some of the opossums.

In his classical paper of 1887, Mr. Oldfield Thomas,[2] after a considerable discussion of the teeth of Marsupials, arrived at the conclusion that originally all Mammalia were wholly monophyodont, the permanent teeth having ancestral priority over their ontogenetic predecessors of the deciduous series.

To account for the supposed secondary development of the deciduous dentition, it was assumed that a retardation of the permanent teeth would be beneficial by preventing overcrowding before the mouth was fully grown, while at the same time some provision of masticatory organs for a young animal with its teeth thus retarded would be essential. This advantageous retarding of the teeth, together with the necessity of finding some temporary grinding apparatus, was supposed to have resulted in the formation of a deciduous set of teeth which are of greater or less complexity, and retained for a longer or shorter time in different members of the Mammalia.

From the fact that only one tooth is replaced in the Marsupials, it was further supposed that the Mammalia had reached a fairly high degree of complexity before the necessity for deciduous teeth became urgent, since they could only have possessed one deciduous tooth when they had already gained all the complications of a generalised Metatherian condition. It was considered that at this juncture the

[2] *Philosophical Transactions* of the Royal Society, 1887.

Marsupials branched off from the main Mammalian stock and obviated their difficulties in other ways than by the development of a complete temporary dentition.

At the time of propounding this lucid and carefully elaborated theory, Mr. Oldfield Thomas pointed out that it would entirely collapse should a more complete replacement be discovered among any of the Marsupials, and he has been the first to acknowledge that this has now been done by Kükenthal.

Years ago Professor Huxley stated his belief that the deciduous dentition now found in Marsupials was but a relic of a fuller milk dentition, instancing an analogous loss in the case of the shrew, which has no deciduous teeth, while its near ally, the hedgehog, has a complete replacement. At the time, this belief seemed barely tenable, since in no Marsupial had any trace of a larger number of deciduous teeth been found, while even the one present in most was known to be absent in some forms (*Phascogale apicalis* and others). Moreover, fossil Marsupials, such as *Triconodon*, show the replacement of the posterior premolar, and of that tooth only. This, however, only affords one more instance of the danger of generalising from purely negative results, for Huxley's belief has now been amply justified by Kükenthal's discoveries, and diphyodontism is proved to have been the ancestral condition of at least the Didelphydæ.

It is, of course, possible that a complete cycle of development has been undergone, the milk dentition at first appearing as a provisional masticatory apparatus and afterwards persisting while their phylogenetic predecessors underwent abortion. There is, however, little or no foundation for such a suggestion; a far simpler explanation is to regard both series as representatives of two consecutive series of the Reptilian teeth.

In making this supposition, we must, moreover, grant some probability that the true molars were at one time subject to replacement, and this hypothesis also receives confirmation from the recent researches on the Marsupials.

Kükenthal appears to have been first led to work at the embryonic dentition of the Marsupials from the consideration of the synchronous development of the deciduous posterior premolar and the remaining permanent premolars. This suggested the idea that all the premolar teeth belonged to the same series, and Kükenthal has succeeded in proving that this is the case by the discovery of rudiments of an almost complete series of successional teeth.

These embryonic rudiments were found, not only for the incisors and canines, but also for the most anterior premolar, and for the first and second teeth of the true molar series. The only rudiments not found were, therefore, those of the second premolar and of the more posterior molar teeth. Further, these vestigial rudiments of all the successional teeth are identical in structure with the developing third premolar, which is afterwards cut, replacing its deciduous predecessor.

The only possible conclusion to be drawn is that in *Didelphys* (and probably in other Marsupials) the teeth of the permanent dentition, with the exception of the last premolar, represent the deciduous teeth of other forms, while rudiments of a second series are present in the embryo, but only cut the gum in the case of the third premolar; these rudiments must obviously be the remnants of a more complete replacement.

If the two series of teeth in the Mammalia are to be regarded as relics of the many series in the Reptilia, the Marsupials, inasmuch as they have almost lost one of these sets, have, in this respect at least, departed further from the common stock of Mammalia than have many of the true placental forms. There is, however, not unfrequently, considerable variation in the extent of replacement, even within the same groups of placental Mammals; for instance, among the Rodents, the Leporidæ have their incisors replaced, while the rat is entirely monophyodont.

A further point of interest lies in the fact that, whereas in the Marsupials it is the second series of teeth, or that which corresponds with the permanent series of most higher forms, which has become degenerate, in other groups it is the precursory ones that are suppressed. We see this suppression of the milk teeth in some of the Carnivora: in the dog, for example, the deciduous teeth are well developed, the bear has much smaller ones, while in the seal they are still more rudimentary, and absorbed before birth.

E. C. Pollard.

V.

The Recapitulation Theory.

A REJOINDER.

IF the value of a theory is to be measured by the number and the extent of the papers written, and by the amount of work done under its inspiration, then the value of the Recapitulation Theory is indeed great: but there is always room for the doubt as to whether an equal amount of work would not have been done without that inspiration; whether, for instance, Hyatt, Buckman, Jackson, and Beecher would not have published researches as great and as brilliant as those referred to by Mr. Bather in his article on "The Recapitulation Theory in Palæontology" (p. 281), even if they had been inspired by something less fantastic than the theory in question.

In attacking this theory, however, I was fully conscious of the enormous amount of work done under its inspiration; and in attempting to destroy the theory, I was fully alive to the possible effect upon the work of the future. I look upon the loss of all the "recapitulation" literature with perfect equanimity—nay more, I look forward with hope for the time when men shall cease to occupy the pages of our scientific journals with controversies as to whether "the ancestor" of the vertebrates was an ascidian, polychæte, nemertine, leech, turbellarian, sea-anemone, sponge, spider, crustacean, Balanoglossus, echinoderm or other *recent* animal of the embryos of which the controversialists happen to have been cutting sections. I do not think that if the writings thus briefly catalogued had been written without the "inspiration" of the Recapitulation Theory, they would have been less valuable. The little I do know about the psychology of "dominant ideas" leads me, indeed, to believe that the investigations would have been carried out, not only more fully, but also with far less error of observation. Under the influence of a dominant idea, men see what does not exist, and are blind to what lies before them; and what is even worse, what little they do see of the things before them is seen distorted. Whether the dominant idea be a true idea or a false one, if dominant, it is almost certain to lead to profound error. When first the idea of evolution gained a hold over men's minds through the influence of Darwin's and Wallace's theory of Natural Selection, it suddenly developed into a *dominant* idea in the

minds of those who accepted it incautiously. It was not the theory which was wrong, but the mental attitude of those who accepted it. It was not accepted as a theory, but adopted as a creed. Fortunately, those who did so adopt it were comparatively few.

With the Recapitulation Theory it is otherwise. *All* who have accepted this, have adopted it *as a creed*. Few of them can even see that it is not an explanation of anything whatever. They *believe* that it explains the series of events constituting ontogeny. In reality, it is only a dogmatic assertion that this series of events is controlled by some mystical "dead hand of the past" in such way as to serve as a record of events long passed. If the dogmatic assertion as to the course of events constituting the ontogeny had proved to be true, then it would be difficult to imagine any more interesting problem than that involved in the *explanation* of so remarkable a phenomenon.

The truth of the assertion has been put to the test, not of direct comparison of the ontogeny with the phylogeny in a large number of cases, for that has hitherto proved impossible (*pace* Bather), but by comparison of the supposed "records" with each other, and by other indirect means. The mere fact that of all the conclusions drawn as to the origin of the vertebrates every one contradicts every other, is one disproof of the theory. The fact that von Baer's law is in exact accordance with observed facts in a large number of cases is another and a more definite disproof. The two views—von Baer's and the Recapitulation Theory—are irreconcilable. Von Baer's is applicable only to certain animals, but its applicability to these disproves "recapitulation" in these. These animals are such as pass through a considerable portion of their development as embryos, or fœtuses, before entering upon the struggle for existence amidst untoward circumstances. The remaining animals, that is, those which have to fight for a living from very early stages indeed, are, by virtue of that early struggle, exposed to the modifying influence of Natural Selection; and, hence, if any "record" had existed in the ontogeny of such species, it must of necessity be very soon obliterated by this action of Natural Selection. The obliteration need not be complete. Some traces of the ancestral ontogeny (*not* phylogeny) may remain, and, so far as these traces have been made out, they also are in conformity with von Baer's law, and, therefore, inconsistent with the theory of recapitulation.

It by no means follows that no ontogeny is such as to present even a startling resemblance to the phylogeny of the same species. Mr. Bather's too brief accounts of the truly remarkable correspondence between what is known to be the ontogeny and what is supposed to be the phylogeny of *Antedon*, will sufficiently impress my readers with this fact, and it is "greatly to be hoped" that he will shortly enrich the pages of Natural Science with a very much fuller account, both of all the stages of *Antedon* and of all its supposed ancestors, *with figures of every one of them*, that we may all more clearly

see what are "the plain facts of palæontology," to which it is suggested (p. 281) that somebody's eyes (? mine) are "persistently shut."

"It is not an occasional accidental parallelism between the ontogeny and the phylogeny which I deny, but the *causal relation* between the two." (I quote from my previous article, p. 198.) This appears to have been overlooked by Mr. Bather, and the whole of his article is consequently directed against a position which I have never held. I have stood perfectly still, and his explosive bullets have rushed past me with a violence suggestive of a shower of meteorites: his aim was excellent; not a single stray bullet touched me.

Mr. Bather admits that "von Baer's law is undoubtedly true" in cases of fraternal relationship, but holds that "there are many objections to supposing that it is equally true" in cases of filial relationship. I had quoted Darwin to show that in the case of the pigeons where the relationship between *C. livia* and the rest is of this kind (the only case I know of which has been tested) the law is fully applicable. May I therefore ask for, say, half-a-dozen of the best of the "many objections" referred to?

In defence of myself against what has now, unfortunately, reached the public eye, I feel bound to correct some of Mr. Bather's misapprehensions as to my position. To each reply I will prefix the number of the paragraph in his article where the misrepresentation occurs.

(3. Middle of p. 276.) The larval stem of *Antedon*, if it be, as I suggested, the modified equivalent of the stalk of the ancestral larva, need not be supposed to be anything other than what Mr. Bather describes, nor yet need any conclusions be drawn from it (as to the stem of its ancestors) which differ in the slightest from the conclusions he has drawn from palæontological work.

(3.) I have not ignored "acceleration of development." The antlers of stags were given as an example of it.

(5. Bottom of page.) Nothing I have said will bear the interpretation here put upon it. Each transient ontogenetic stage may be a modified equivalent of a corresponding ontogenetic stage in an ancestor, and may yet be either more complex or less complex, or may even be so completely changed that no resemblance is observable.

(5. P. 277.) I do not feel bound to suppose, and have never supposed, anything at all comparable with what is here suggested. I do unhesitatingly say that the ontogeny described in *Antedon* is not an epitome of the ancestral *history*. There is a truly remarkable parallelism between the ontogeny and the phylogeny as here set forth. It is not a history, inasmuch as the correspondence between the two series is not one which could safely be *assumed* to exist apart from palæontological evidence.

(7.) Let it be assumed that the evidence set forth by Darwin

proves conclusively that the domesticated breeds of pigeons are descended from a wild species in no way differing from the existing *C. livia*. Then variation (in my narrowed sense of the term) has so affected the adult structure, under the influence of long-continued artificial selection, as to produce barbs, runts, fantails, &c. Direct observation of late stages of development in each of these breeds has shown (Darwin) that these stages also have varied in the *same* direction, though to a smaller extent. In each of these breeds the newly-hatched nestling is *less* like the adult *C. livia* than is the newly-hatched nestling of the "wild parent species," *i.e.*, the *C. livia* of to-day. In order to produce a "record" of the descent from *C. livia*, *i.e.*, in order that the late stages of development should reproduce the *adult* characters of *C. livia*, those late characters would have had to vary in the direction of *greater* likeness to the adult *C. livia* during the same time as the adult characters were varying in the direction of *less* likeness. That is what I have called varying in "another" direction. I did not say "opposite" direction, for the simple reason that stags' antlers, &c., were in my mind at the time, and it was obvious that, in such exceptional cases, the two directions were nearly or quite parallel.

(8.) I have never either denied or hesitated to admit the occurrence of such phenomena as Mr. Bather records in the Ammonites; nor do I dispute the *facts* made known by Würtemberger, Waagen, Branco, Hyatt, Buckman, or anybody else. What I have denied I will deny afresh at the end of this article.

(9.) I find no fact whatever (not even an assertion!) in the table given on p. 279 which in any way runs counter to any view I have put forward.

(10.) I have paid full attention to the opinions of palæontologists so far as I have become aware of them, but I bow to no *opinion* based upon an argument which is inconclusive. The mere opinion of the whole zoological and embryological world appears to be opposed to mine. Would Mr. Bather wish me to bow to that opinion also?

(11.) What Mr. Bather calls my "main fallacy" is one into which I have never fallen! I have never believed that existing species are descended from other *existing* species, except under the influence of artificial selection (as in pigeons), or geographical or other isolation.

(11.) All seven parts of the quotation tell equally in my favour. Not one tells against me.

(12.) Whatever I may seem in Mr. Bather's eyes to suppose, I can assure him that I never supposed any reasonable person to hold the Recapitulation Theory in any form demanding such a "harlequinade" as he describes. It was not in any such form that I either described or attacked it.

(13.) The reason for introduction of *Culex*, etc., into my paper will be obvious to anyone who will read the paragraph which

mentions them. (P. 196.) It begins with the words "Another kind of variation is illustrated . . . . " By reading to the bottom of the same page it may be seen how I "made capital out of them."

(14.) "Another mistake" of mine (as Mr. Bather calls it) is like my "main fallacy"! Not only did I *not* refuse to admit more than one kind of variation, I actually insisted on the necessity of recognising *at least* two kinds. (P. 196.) I am fully aware that each "kind" includes many varieties.

(14.) The remainder of this paragraph is unintelligible to me. "The very same '*change in the constitution of successive generations* of a species leading to the production of a new species' *does take place in the life-history of a single generation.*" And again :—"All these well-known instances of variation (and I use the term precisely in Mr. Hurst's own sense) 'occur in a way utterly unlike the way in which it does actually occur,' *i.e.*, within Mr. Hurst's horizon." What can this mean?

(15.) By making myself acquainted with certain brilliant researches "inspired by the Recapitulation Theory" to which Mr. Bather directs my attention, I have no doubt whatever that I may learn much. That the brilliant researches should have been inspired by that theory will certainly *not* make me reconsider my condemnation. The whole science of chemistry has arisen out of brilliant researches inspired by the theory of the transmutability of the baser metals into gold. Shall we therefore accept that theory? Mr. Bather is hopelessly at fault in his assumption that my attack on the theory is due to my ignorance of the many brilliant discoveries which have been made under its inspiration.

The "amended" statement of the method of variation given on p. 277 errs in the precise way in which Mr. Bather says that my paper did. It refuses to admit more than one kind of variation! Moreover, it speaks of events being "*pushed back*" *in time* by other events which have not yet happened. This is, no doubt, figurative, but whether any such erroneous conception exists in Mr. Bather's mind or not, the acceptance of the statement in that form by recapitulationists would certainly produce in the minds of the "*profanum vulgus*" an impression that this was a case in which the effect is produced before the cause has arisen.

To prevent any further misrepresentation as to what I have denied and what I have not denied, I will conclude with a summary statement of my position :—

I do not deny that a rough parallelism exists in some cases between ontogeny and phylogeny. I do deny that the phylogeny can so control the ontogeny as to make the latter into a record of the former—even into an *imperfect* record of it.

I assert that those very characters of the adult which vary most in the adult are precisely the same as those which vary most in the late stages of development also.

I do not deny that there may be *exceptions* to this last statement.

I deny that any one of the theories as to the ancestry of the vertebrates based exclusively on embryological grounds is worth the paper it was written on.

I maintain that much otherwise good embryological work has lost much of its value through the bias produced in the minds of the writers by the dominancy of the idea of recapitulation.

Comparative anatomy gives some help: Palæontology may give even more: by their aid embryological facts may be explained: but the facts of palæontology can never be discovered by the study of embryology.

"Vestiges," and these only, can give any embryological clue to past history which could not be equally well made out from comparative anatomy. In this last I may be wrong.

I maintain that the applicability of von Baer's law to even a single case would be a definite and final disproof of the Recapitulation Theory even in its most reasonable forms.

Finally, I assert that the applicability of von Baer's law in a large number of cases is capable of being readily proved by anybody, by a direct appeal to Nature, *i.e.*, by the study of a few vertebrate embryos taken at random.

C. Herbert Hurst.

VI.

Climate and Floral Regions in Africa.

ONE of the strangest features of African botany is the extreme poverty of species within the tropics, accompanied by a wealth and superabundance of vegetation, *i.e.*, of individuals, which can be hardly realised without actual observation. On the other hand, outside the tropics, both the number of species is astonishing (sometimes there are more known from a given area than is the case anywhere else in the world), and again, the number of individual plants on a given area is exceedingly small as compared with the tropics, and never shows the same extraordinary luxuriance.

The reason for this difference may be partly appreciated by noticing some of the distinct climatic conditions which exist in temperate Africa; each different climate maintaining, of course, its own set of plants.

Travelling in imagination along North Africa, from Gibraltar to Egypt, we notice the following changes. First, in Morocco, Algiers, and part of Tunis (roughly speaking, on the Northern Mediterranean side of the Atlas chain and its continuations, which run out in the sea near Tunis) we find ourselves in a Mediterranean climate. It is a fairly dry country, but does not suffer from drought to any severe extent. On an Algerian hillside, one sees numerous wild olive trees, many bulbous plants, and small shrubby perennials, while in sheltered places many annuals occur, but there is an almost complete absence of thick sward and the dense growth which one notices, for instance, in England. There is nothing like the crowding and struggle for existence which goes on in temperate Europe; every plant seems, as a rule, able to spread out, and the tint of the landscape is that of the underlying soil, often red, with a sort of grey-green haze, due to the scanty vegetation; the number of species is, however, fairly large. An examination of the plants themselves shows that they are mainly Mediterranean forms with numerous endemic species or varieties, whose origin from European species or their ancestors is intelligible enough if we remember the years that have elapsed since Tunis was in free land communication with Sicily, and since the Straits of Gibraltar were formed.

In Tripoli and the Bay of Syrtis, we are, in the desert of Sahara, exposed to the most severe and protracted drought. The plants that exist are very isolated indeed, in most places a foot or more apart,

and there are no plants whatever over wide areas. In spring, a considerable number of small annuals start up after the rains, flower and fruit in an incredibly short time, and die down immediately the country dries again; the residents are stunted, very often thorny bushes, extremely small and only able to resist drought either by covering themselves with a woolly coat, or by hiding their transpiration pores in cunningly-concealed pits or grooves which are so many traps to prevent the egress of the precious water. Some are said to surround themselves with an atmosphere of their own by exudation of essential oil, &c., but this is theoretical. Many have no leaves, others only have leaves in the wet season, and the few that keep their leaves are succulents or have them most beautifully adapted to their requirements.

When we proceed further along the coast and reach the little limestone hillocks by the sea at Alexandria, we are again among a different set of plants. They are very near the Algerian forms, but not exactly the same; probably they came from Palestine and are part of the Syrian flora as distinguished from that of the desert. Nothing is more exquisite than the flora of these nummulitic limestone hills. There are masses of the crimson *Ranunculus asiaticus*, the delicate little white star-of-Bethlehem, many species of *Astragalus*, and generally a wealth of colour and a beauty of form which I have never seen elsewhere.

On entering the Nile delta there is again a sudden and distinct change. The deep, carefully-irrigated alluvium is covered with a dense mass of plants, that is, if they are allowed to grow, for the soil is very valuable, and these very special characteristics of soil and water have produced the endemic Egyptian plants, such as the *Trifolium alexandrinum*, or "berseem" clover, of which some seven crops can be raised in one year on the same ground. Travelling up the Nile, we find ourselves before Assouan out of the delta flora, and again, as at Tripoli, in the great desert whose plants in many cases extend from Beluchistan to the shore of the Atlantic between Morocco and Senegambia.

Thus, in North Africa there are four marked floras more different from one another than are the floras of England, and one might almost say, Tibet, and to these must be added that of the higher peaks of the Atlas Mountains, which is of an alpine character, that is to say, five different sets of plants in Extratropical North Africa.

In Extratropical South Africa, beginning at the North, we find along the coast an offshoot of the evergreen humid tropical forest which has crept down from the Zambesi, and has occupied Pondoland, the lower parts of Natal, and, in an extremely modified condition so far as species go, formed the Perie Forest of King William's Town, and the Knysna Forest of Cape Colony. All these species of the eastern littoral, finding themselves in temperate and not tropical conditions, have varied greatly, and being separated from each other,

the forests of lower Natal, the Perie, and the Knysna have each their own endemic forms. Each is, in fact, a climatic island, and varieties suited to very local conditions have free play.

Further inland the plateaux of Mashonaland, the Transvaal, and upper Natal are covered with grass, among which are perennials and bulbous plants, and in places isolated shrubs or small trees, chiefly acacias. It is believed that there must be a communication with Abyssinia by ridges or mountain summits, at least 5,000 ft. high, in the tropics, and that the plants have travelled down along these ridges and summits without experiencing any great climatic change on the way, but this is rather theoretical at present. At any rate, they have taken on different specific characters *en route* in most cases, and they are quite different from those of the coast below 2,000 ft. Following the plants of these grassy plains southwards, we can trace them, often in very modified forms, on isolated mountain summits (such as at Somerset East) as far as the top of Table Mountain at Cape Town; but within the Cape Colony these grassy mountain tops, which are usually flat and table-like, are also inhabited by a wealth of endemic Cape genera and species, which probably ascended the hills from the lower parts of the peninsula. Every isolated mountain top may be in this way a climatic island, with species of its own. Still these plateaux and tables are generally in a fairly moist climate, and under quite different conditions to either the arid deserts of the Karoo or the western parts of Cape Colony and the lower parts of the peninsula.

In the Karoo and Namaqualand similar conditions prevail to those of the Sahara, and we find there bulbous plants, succulents, thorny plants, and densely matted perennials, similar in structural type to those of the Sahara, but of utterly different origin. The Cruciferæ and Chenopods, which are predominant in the latter, are almost absent here, and their place is taken by Mesembryanthemums, Stapelias, Pelargoniums, and others, which are not found in the north, and make a very different flora. Here, again, we find the landscape the colour of the soil, and scarcely tinted by the vegetation.

As for the distinctive Cape flora (that is to say, the remainder after the Karoo-Namaqualand and mountain-flora has been subtracted), it is different to any of the others; the conditions are rather dry, and yet not desert ones; the rainfall occurs at a different season to that of the eastern slopes and the Transvaal, and the prevailing type of plant is again different. Most are shrubby little perennials like the heaths, and a very large number have heather-like leaves; there are many bulbs, and scarcely a single annual of any sort or kind. The landscape is not wholly free from the colour of the soil, but it is masked by the grey-green tints of these little shrubs, and sometimes it is bright and rich in colour, as when *Erica Plukenetii* is in bloom. It is never, however, so beautiful as, for instance, that of the top of Table Mountain in the right month, which simply beggars descrip-

tion, and the conditions vary so greatly that in this peninsula there are probably more species than exist anywhere in the world on a similar area.

This makes, therefore, in Southern Extratropical Africa, four distinct floras, all, probably, closely united genealogically, but all specifically and very often generically distinct.

Turning now to Tropical Africa, a glance at the map and the exercise of the imagination will show that the enormous valleys of the Congo and Niger, up to from 3,000 to 5,000 feet, have been excavated out of a continuous tableland, and are in free communication with each other by the littoral of the west coast. Speaking generally, the whole of this enormous region is one continuous, dense, evergreen forest, and similar humid conditions prevail throughout its whole area. It, therefore, forms another flora comparable with the others and probably not nearly so much richer as might be supposed. The higher lands above 3,000 feet at 10° N. lat., and above 5,000–7,000 feet at the Equator, have another flora of a more or less similar type to that of Abyssinia, and the truly alpine flora of the Cameroons, for instance, seems almost specifically the same. In this region one can never see the landscape, as one walks usually through hedges of trees and shrubs 30 to 70 feet in height, or when the forest has been cleared, through tall grass which gives a realistic idea of the presumable view of a rabbit in a field of corn.

Hence, to show the relative abundance of floral regions, we have in North Extratropical Africa the South European flora in Morocco and Algiers, the Sahara desert flora, the Syrian flora at Alexandria, and the Egyptian or Delta flora, while in Southern Extratropical Africa there are temperate forest floras in Natal and the Cape, a grassy plateaux flora in the Transvaal and Cape Colony, the Karoo desert flora, and that of the Cape proper; that is to say, eight different sets of climatic conditions. In Tropical Africa there are the evergreen forest and the plateaux flora, that is to say, only two, of which the second is practically the same as that of the Transvaal, or is, at any rate, so close as to be scarcely reckoned as distinct. If one takes in the alpine flora of the highest summits of the Atlas, Abyssinia and Kilima-njaro, we get a total of eleven different floras.

The manner in which these have so far been studied reveals the difficulties of systematic botany. Boissier's "Flora Orientalis" mixes up part of the Algerian, part of the Sahara and part of the Syrian and Egyptian floras. Oliver's "Flora of Tropical Africa" includes the tropical forest, the Abyssinian and Transvaal grassy plateaux, and the alpine plants in part, and is about half done. Harvey and Sonder's "Flora Capensis" mixes inextricably the grassy plains of the Transvaal, the Karoo, and the peninsula, and is not half finished. Engler's "Hochgebirgeflora," containing the high mountain plants, is the only attempt to work on natural lines, and is also pre-eminently distinguished from the last two by having reached a conclusion.

G. F. Scott Elliot.

VII.

The Moas of New Zealand.

IN a paper in vol. xxiv. of the *Transactions of the New Zealand Institute*, p. 93, Mr. F. W. Hutton has discussed at great length the classification of the Moas of New Zealand. "My work," he says, "is founded on the measurement of the leg-bones of individual birds belonging to sixteen different species, and from these I have inferred with considerable certainty the proportion of the leg-bones in the other species." As to the genera, he says, "the crania of the different Moas are quite sufficient to indicate the existence of several genera. . . . After reducing the species to order, I find that they fall into seven well-defined genera founded on the crania, but generally accompanied by characters derived from the pelvis, the sternum, the absence or presence of a scapulo-coracoid, and the robustness of the leg-bones" (*loc. cit.*, p. 100).

In his paper Mr. Hutton gives a table in which the *Ratios in the Genera* are obtained, *as regards the long bones*, by dividing the length by the girth; and *for the skull* by dividing "the breadth at the squamosals" by "the height at the basitemporal," called ratio of breadth to height, and "the length from the supra-occipitals to the nasals," by "the breadth at the squamosals," to obtain his ratio of length to breadth. On examining this table we find that the maximum ratio of length to girth of the metatarsus in *Dinornis* is 3·6, and the minimum 2·5; while the maximum and minimum ratios in *Tylopteryx*, *i.e.*, 3·3 and 2·7, *both* lie within the maximum and minimum assigned to *Dinornis*. The same is the case with their tibiæ and the femora, so that as regards their long bones, those placed in *Tylopteryx* could all have gone into the genus *Dinornis*, for there is no reason why they should be assigned rather to one skull than to another. If we take the ratios of the crania (assigned to a great extent, therefore, arbitrarily to these limb-bones), the maximum and minimum ratios of breadth to height and of length to breadth in *Dinornis* fall completely within the ratios of *Tylopteryx*, and those of the sternum and pelvis of *Tylopteryx* fall within the ratios of *Dinornis*.

The ratios of the maxima and minima of both the metatarsus and the tibia of *Palapteryx* (the next genus further removed than *Tylopteryx* from *Dinornis*), fall also within those of the latter genus, so that as regards the metatarsi and tibiæ of *Dinornis*, *Tylopteryx* and

Palapteryx, there is no apparent reason for separating them. Take the next genus, *Anomalopteryx*, the maximum ratios of both its tibiæ and femora fall between the maximum and minimum of the genus preceding it, while the maximum ratios of the metatarsus, tibia, and femur of *Cela*—the genus following in succession—fit between the maximum and minimum of those in *Anomalopteryx*, while the ratios obtained from the metatarsus, tibia, and femur, in the next genus still, *Mesopteryx*, fit also between the highest and lowest ratios in *Cela*. It is evident that these ratios are valueless for differentiating the various bones into their proper genera *by measurement* alone, as is done in this paper. Indeed, if Mr. Hutton has correctly assigned to the genus *Euryapterx* three types of skull, why may not many of the varieties or types of cranium of Moa which we know not have belonged to one genus, even species, just as the cranium of *Gallus* or *Columba* does? It is also only in a few cases known certainly to what crania the numerous separated beaks belong, most of them having been only *fitted* together.

Turning to the question of species, the metatarsus of *Dinornis maximus* is separated from that of *D. excelsus* by extreme difference of ·5 inch in length and 1·25 in girth, and by a mean difference in the two species of ·5 inch in length and of ·7 in girth; while associated together as *D. validus*, we have metatarsi varying as much as 1 inch in length and ·8 inch in girth. The difference between the minimum tibia of *D. maximus* and the minimum tibia of *D. excelsus*, is 1·5 inch in length and 1 inch in girth, while we have tibiæ differing by 1·5 inch in length and in girth of 1·2 inch assigned to the one species, *D. validus*. Again, the maximum metatarsus assigned to *D. giganteus* lies midway between the greatest and least metatarsus assigned to *D. validus*; the minimum tibia assigned to the former is intermediate between the largest and smallest of the latter; while the maximum femur of *D. giganteus* is identical with the minimum femur of *D. validus*. The greatest difference in the metatarsi between the largest *D. validus* and the smallest *D. giganteus* is only ·5 of an inch in length, and 1·8 in girth; in the tibiæ 2 inches in length and 1·2 in girth; in the femora 1 inch in length and 1·4 girth. If we compare *D. robustus* with *D. firmus*, we find,

	In *D. robustus*.		In *D. firmus*.		A mean difference of	
	Length.	Girth.	Length.	Girth.	Length.	Girth.
Metatarsus ..	17·2–15·7	6·7–5·2	17·0–15·75	5·6–4·75	·08	·78
Tibia	32·7–30·0	6·8–6·2	33·0–30·0	6·5–5·3	·35	·60
Femur	15·5–14·4	8·1–7·2	15·25–14·5	7·5–6·0	·07	·90

Take another instance, and compare *D. ingens* with *D. firmus*:—

	D. ingens.		*D. firmus*.		Difference.	
	Length.	Girth.	Length.	Girth.	Length.	Girth.
Metatarsus ..	15·25	4·75	15·75	4·75	·50	·00
Tibia	29·50	5·30	30·0	5·30	·50	·00
Femur	14·25	7·25	15·25	7·50	1·0	·25

Is there any real reason why these metatarsi, tibiæ, and femora

should not all be assigned to the same species? Mr. Hutton has in his paper placed bones that vary by only ·5 of an inch in length in *different* species, while in the case of *D. validus* above, we have metatarsi differing by 1·5 inch in length and 1·2 in girth united in the same species. Why also should metatarsi whose dimensions lie between

12·2–13·0 ins. in length and 4·0–4·75 in girth be described as *D. torosus*.

those between

13·0–14·25 ,, 4·25–4·9 ,, ,, *D. gracilis*.

and those of

12·0 ,, 5·0 ,, ,, *D. struthioides*.

and why should femora of

11·50 ,, 5·5 ,, ,, *D torosus*.

and those of

11·25 ,, 5·5 ,, ,, *D. struthioides*.

We find in this lengthy paper also the following :—

Metatarsi measuring 10·4 inches in length and 4·0 inches in girth = *dromioides*
and ,, ,, 10·6 ,, ,, 4·0 ,, = *plenus*
giving a difference of 0·2 in length and ·0 in girth.

Tibiæ measuring 21·0 inches in length, and 4·0 inches in girth = *dromioides*
and ,, ,, 21·5 ,, ,, 4·6 ,, = *plenus*
giving a difference of ·5 in length and ·6 in girth.

and Femora measuring 9·6 inches in length and 4·0 inches in girth = *dromioides*
,, ,, 10·0 ,, ,, 4·0 ,, *plenus*
Difference of ·4 in length and ·0 in girth.

How unsatisfactory this method of classification is, may be gathered from Mr. Hutton's observations under *Cela curta*, Owen, where we read: "I regret that I cannot maintain *D. oweni* (described by Von Haast) as a separate species. It is only a small individual of *C. curtus* . . . Sir J. V. Haast says that the femur is shorter and the metatarsus more slender in *D. curtus* than in *D. oweni*; but my measurements do not bear this out, and the supposed anatomical differences between them are only individual variations, which may be found in almost every species of Moa. If *oweni* is to be separated from *curtus*, then for the sake of uniformity most of the species should be split into two, for they show quite as wide a range of variation." Now of *D. oweni*, the *metatarsi* are ·6 of an inch in length by ·5 inch in girth, and the *tibiæ* 1·65 inch in length by ·35 inch in girth less than *C. curta*; but the metatarsi and tibiæ of *P. dromioides* and *P. plenus*, which differ by a much less fraction from each other, are placed under different species. Nevertheless, it has been found necessary to unite Haast's species, *D. oweni*, though separated by greater differences, to *Cela curta*. It is very difficult to comprehend on what grounds these unions and separations are based, and impossible to assign unknown bones to their proper genera and species solely by this method of differentiation.

As for the identification of *Anomalopteryx antiqua*, it is founded on the photographed pieces of a metatarsus which Mr. Hutton had no opportunity of examining, and on fragments of two tibiæ, which

were, as I was informed by the mine-driver who excavated them, obtained from tunnels far apart, and many feet further under the lava, at a different part of the quarry from where the metatarsal fragments were obtained. These fragments are quite insufficient to determine with anything like the accuracy employed throughout his paper by Mr. Hutton the length of the tibia, which is, notwithstanding, entered (without a mark of interrogation as if certainly ascertained) as 12 inches. The chances are almost infinite against the two pieces being parts of the same bone. In speaking of these two fragments of tibiæ (which I examined carefully in the matrix, but found too imperfect to dare to identify), I say that they "were, undoubtedly, portions . . . of one of the greater forms." I should have expressed myself more accurately if I had said they were "undoubtedly not one of the smaller forms." As regards the metatarsal fragments which Mr. Hutton has assigned to this species, apparently for the reason that they were found in the same quarry, I have carefully compared them with the collection in the British Museum, and they agree most closely in form, though larger in size, with those of *D. curtus*. They belonged, however, *most certainly* to a species which was *not* "smaller than any which lived subsequently in the south island," as Mr. Hutton affirms. It cannot, therefore, yet be argued from them that the earliest known Dinornithidæ were smaller than those that succeeded them.

The differentiation of Moa bones into genera and species solely by the differences in their length and girth, I hold to be quite unreliable and misleading. Their form and outlines, I venture to think, are the only characteristics by which they can be accurately separated from each other. Before leaving New Zealand, I had already commenced to protract on paper the prominent features of the principal groups by referring them to rectangular co-ordinates which were drawn through the corresponding points of all the bones. By this method, I found that their points of agreement and difference, quite apart from size, could be strikingly exhibited. Professor T. J. Parker has recently contributed to the Zoological Society in a paper to be published in its *Transactions*, a classification of the Dinornithidæ, solely based on the crania of these birds; and as he hopes, I believe, to supplement it by a paper on the remaining bones of the skeleton, I trust he may give the suggestion I have made here a trial. The method will, at all events, I think, be found to afford an invariable standard of reference in describing the different bones. His forthcoming paper has, it is satisfactory to know, been collated with Mr. Lydekker's classification in the British Museum Catalogue of *Fossil Birds;* while, on the other hand, it is greatly to be regretted by all workers on this most difficult subject that Mr. Hutton did not defer the publication of this valuable paper, in which has been brought together almost all the known information on the Moa, till he had found time to compare his nomenclature with

that of Mr. Lydekker's catalogue,—a volume which had already reached the Colony before the reading of his paper, in which the classification is based on the type material in the British Museum, instead of adding to the already almost hopelessly involved synonymy of the Dinornithidæ, as his paper before the New Zealand Institute unfortunately does.

As to the age of the oldest fossil Moa bones found in New Zealand, Mr. Hutton, I find, dissents from my opinion as to the geological horizon in which the remains were found, which he is, of course, entitled to do. In stating, however, that "this conclusion was not arrived at by a re-examination of the sections at Mount Horrible and the Pareora, Mr. Forbes merely went to a quarry near Timaru, and, with 'little doubt,' identified a 'rough red shingle,' which he did not even see *in situ*, with the gravels of the alluvial fans of the Canterbury Plains," Mr. Hutton has affirmed what is not only quite incorrect, but what could not possibly be within his knowledge. He could not know how often I examined the geological structure of the region, or with what care, or what I have seen or not seen *in situ*. The shingle below the clay (or so-called loess) underlying the lava-bed, by which the age of the section is determined, I practically *did* see *in situ* on my first visit, and having subsequently re-examined the district, I have assured myself of the correctness of my previous observations and opinion. I shall, however, most willingly admit that I am mistaken as to the age of the strata in which these *Dinornis* remains have been found as being other than "newer Pliocene or even Pleistocene," so soon as the officers of the New Zealand Geological Survey—who are really the only competent referees in the case—shall have assigned to these gravels a different age. Notwithstanding Mr. Hutton's doubt as to the correct identification of the avian remains in the same bed in association with those of the Moa, I can, without hesitation, re-affirm that the bone I determined as *Apteryx australis*, and figured in the *Transactions* of the New Zealand Institute (vol. xxiii., pl. xxxvi., p. 368), really belonged to that bird. The bone, of which the drawing (for which I am responsible) is unfortunately not so good as a better draughtsman might have made, was, as I there pointed out, somewhat distorted by heat, but otherwise its identity was unmistakable. I am glad to find that Mr. Hutton admits "if it had been correct that bones of a living species of Kiwi occurred with them [the Moa-bones], it would have been strong evidence in favour of the bed being younger than Miocene." His observation that the figure in the plate illustrating my paper more resembles the femur of *Aptornis* surprises me, for the bones in the two birds are very dissimilar in form and strikingly different in size. The bone figured by me is represented of the natural size, and even in so poor a figure, the delineation of its internal structure recalls at once the section of an *Apteryx* femur.

Mr. Hutton, in the same paper, discusses the question, How long

ago did the Moa became extinct? My exploration of the Sumner Cave proved conclusively that the Moa and the Maori were contemporaneous (*Trans. N. Z. I.*, vol. xxiii., p. 373). This cave had been shut up by an extensive landslip, but for how long a period it was impossible to discover any evidence. The carving on the implements left on its floor was unmistakably done by Maoris. Along with the remains of Moas and other birds round the last fire-place of the inhabitants, I found the shells of Moas' eggs, with the shell membrane still intact, showing that the shells had not been part of any utensil, and their presence in the cave at all seems to leave little doubt that they were brought in as eggs for the purposes of food.

Now it is held by some authorities on Maori history and tradition that, because the Moa is rarely mentioned in their poetry or proverbs, this knowledge "dates from . . . . almost prehistoric times, long before the beginning of the genealogical descent of the tribes, which, as we know, extended back for more than twenty-five generations" (Colenso, *T. N. Z. S.*, vol. xii., p. 63). On the other hand, the Maoris now or recently living, especially in the North Island, have numerous traditions about the Moa, its colour, feathers, food, manner of life, fighting, and about their mode of capturing it. Mr. Hutton says, "So far as the North Island is concerned, I am compelled to believe that the Moas were exterminated many [400-500] years ago, because I feel sure, if it were not so, we should find as many allusions to it in Maori tales and poetry as we do to all the other birds, beasts, and fishes that were of interest to the natives." The date of the extinction of the Moa, I feel, however, cannot be reliably determined by reference to the native traditions or proverbs, for in Monck's Cave at Sumner, associated with the remains of the Moa, I discovered, for the first time, evidence of the former existence in New Zealand of a species of swan, somewhat exceeding the Black Swan of Australia (*Chenopis atrata*) in size.

Shortly afterwards, I received from various correspondents (chiefly from Mr. Hamilton, now of the Otago University), and from all parts of both islands, abundance of evidence that this bird had been at one time widely distributed, and had been used in great numbers by the Maoris for food down to comparatively recent times, for their remains were found in middens, precluding the possibility of their being of vast antiquity. In the Chatham Islands, also, I found, in abundance, bones of what I take to be the same species of swan, in ancient kitchen middens of the Morioris, in association with the remains of *Aphanapteryx*, and from localities there, as well as from the state of their preservation, which also precludes the idea of any great age. Yet though so abundantly used as food by these peoples, and especially by the Maoris, who have handed down, with great fidelity and exactitude, the names of most of their food animals, with their mode of capture, their traditions are entirely silent about this conspicuous bird, and, till two or

three years ago, no Maori scholar had even a suspicion that the swan was known to the natives before its quite recent re-introduction by Europeans, and if this omission from their traditions has taken place with one important bird, why not with another? In this case, the negative evidence relied on by Mr. Hutton is far from being convincing.

The enormous number of bones found by the early settlers scattered over the surface of the ground on both islands, which have now nearly disappeared, is evidence which seems to me to prove that the Moa existed down to quite recent times. This disappearance of the bones is attributed by Mr. Hutton to "the constant burning of the scrub by *Europeans* [for he evidently thinks (p. 151) that there were no fires before there were "human inhabitants to light the fires"], in which case the surface bones do not prove the late existence of the Moa in the districts where they were found." In the time (according to Mr. Hutton 400–500 years) since the Moa disappeared, there must have been all over the country fires ignited hundreds of times by lightning, and by stones rolling down hill-sides striking sparks from other stones and setting the parched grass, which is as inflammable as tinder after every nor'wester, on fire, which would travel farthest and burn most furiously just in those dry regions of Otago, where, Mr. Hutton believes, "bones, skin and ligament, once dried, and protected from the sun might easily be preserved for centuries." Then, also, the numerous little heaps of gizzard-stones lying on the surface of the ground (evidently voided by the birds), can, surely, scarcely have remained so little disturbed, even where protected by vegetation, for four or five centuries, in the midst of the denudation that goes on in New Zealand, by rain, frost and winds; for the gales that blow from the north-west in the summer can carry pebbles, as I have myself seen, larger than most of the Moas' gizzard-stones. It is very singular that Polack (whose book was published in 1838) should have been told by the natives that large birds (which he considered to be struthious) were then still living in the South Island, and this, it must be remembered, was before there was any suspicion of the existence of the Moa, and before the first bones had been discovered; it is not improbable, therefore, that the Moa may have been living on the South Island down even to the time that Captain Cook visited New Zealand.

Much, it is evident, still remains to be done before it can be said that the life-history of the Moas has been fully elucidated or their classification satisfactorily established.

HENRY O. FORBES.

SOME NEW BOOKS.

Types of Animal Life. By St. George Mivart. Small 8vo. Pp. viii. and 374. Illustrated. London: Osgood, McIlvaine & Co., 1893. Price 6s.

Authors sometimes fail to do themselves justice by not selecting a sufficiently comprehensive title for their productions, but this omission certainly cannot be charged against Professor Mivart in regard to the present work. When we first opened the volume before us we not unnaturally expected to find a work somewhat on the lines of the late Professor Rolleston's well-known "Forms of Animal Life," in which typical representatives of the leading groups of animals of all kinds would be treated in the author's well-known style. Our surprise was accordingly great when we found that "Types of Animal Life" really meant, at the most, "Types of Vertebrate Life," and chiefly "Types of Mammalian Life." As a matter of fact, the work is exclusively restricted to those groups of Vertebrates possessing limbs differentiated into the full number of segments, and consists mainly of essays on the leading groups of Mammals; certain chapters being devoted to Birds, Reptiles, and Amphibians—apparently with the intention of pointing out the essential differences of the members of these groups from Mammals. As a whole, the work may be regarded as a companion to the author's recently-published "Elements of Ornithology," with a chapter on Birds which, in our opinion, might have been advantageously omitted.

The author commences his subject with a well-written dissertation on monkeys, as the representatives of the highest order of Mammals; and in the next chapter takes the opossum as his text for a sermon on the lower members of the same class. The next three chapters are devoted to the representatives of the lower Vertebrates coming within the scope of the work, namely, Birds, Reptiles, and Amphibians; while in the sixth chapter the author reverts to Mammals, taking the Carolina bat as a typical example of the Chiroptera. Then follows a chapter headed the American bison, in which the Ungulates are dealt with; while another, under the title of the racoon, introduces us to the Carnivores; and a third, designated the sloth, treats of the Edentates. In the tenth chapter, the title sea-lion appropriately designates a dissertation on the seals in general; while of the two remaining chapters, one is devoted to whales and sirenians (called by the author "mermaids") and the second treats of such Mammals as are not mentioned in the foregoing sections. The last chapter, by the way, is entitled "the other beasts," which leads us to remark that, in our opinion, it is decidedly a pity the author has thought fit throughout the work to follow the lead of the late Professor Parker in employing the objectionable term "beasts" in place of the now familiar mammals.

The work is fully illustrated and pleasantly written, and is admirably adapted for such readers as wish to gain a general idea of

the leading types of mammalian life, and their relations to birds, reptiles, and amphibians, without being encumbered with descriptions or systematic arrangement. The author has, moreover, exercised a wise discretion in avoiding, so far as possible, the use of technical names, which renders the book far more pleasant reading than several popular works we could name, and it is almost needless to mention that he is in general a master of style. We venture, however, to suggest that the less frequent use of the word "nevertheless" would save some weariness to the reader, and that when it is used the punctuation ought always to be the same (see pp. 150, 151).

The mention of punctuation reminds us that Professor Mivart stands in sad need of a proof-reader, not only in the present, but in some of his other works. For instance, on page 60 we are informed that the marsupial mole hails from America, while on page 182 we are told that tame gayals exist in certain parts of Spain! Again, on page 349 Soumerat stands for Sonnerat, while, if we mistake not, on page 292 Seaman does duty for Scammon. Moreover, it would have been well if the author had made up his mind on what plan he intended to transliterate Greek names, and we should not then have had *Chirotes* in one place (p. 146) and *Cheiromeles* in another (p. 165).

There are, however, other and more serious errors in the book which cannot be laid to the charge of the long-suffering printer, and for some of which the author will find it difficult to plead any adequate excuse.

One of the most glaring of these blunders occurs on p. 3, where the author states that the American monkeys, as compared with their Old World allies, "have developed an additional wisdom-tooth." And again, on page 32 we read that "the monkeys hitherto noticed, whether they have two or three wisdom-teeth on either side of each jaw, all agree with us in having three grinders, which have milk predecessors, and are technically known as premolars. But the marmosets alone have only two such on either side of either jaw." Now, in the first place, it is totally inadmissible to speak of all the molars as "wisdom-teeth," that name applying solely to the last of the series. Next, with the exception of the marmosets, all monkeys and apes, like ourselves, have three molars (wisdom-teeth); while those of the Old World have two premolars. The New World monkeys differ in developing a third premolar; while the marmosets are distinguished from these by the loss of the last molar, a true "wisdom-tooth," and thus have three premolars and two molars, and not, as the author states, two of the former and three of the latter teeth. Such a hopeless muddle as Professor Mivart has made of a simple subject would be hard to equal; and it is doubly to be deplored in a work intended to instruct the weaker brethren. It is, indeed, instruction (!).

We have already mentioned two slips in regard to distribution which may probably be attributed to careless proof-reading; but what are we to say with regard to the statement on page 184 that "the goats are exclusively confined to Southern Europe and Northern and Central Asia"? Has the author never heard of *Capra siniatica* of Egypt and the Sinaitic Peninsula, of *C. walie* of Abyssinia, or of the Nilgiri wild goat? Then, again, the statement on page 320, that the common rorqual is the largest mammal, is manifestly incorrect, the creature which has the honour of occupying this position being the blue whale (*Balnæoptera sibbaldi*).

Other statements show want of reading on the part of the author.

For instance, we find it mentioned on page 9 that the apes obtained during Hanno's "Periplus" were probably gorillas. Now it has been conclusively shown that the place where Hanno touched was Sherboro Island, in north latitude 7° 30″, or 4° 60″ north of the range of the gorilla! If the creatures then obtained were anthropoids at all, it is, therefore, evident that they must have been chimpanzees. So far, however, as we are aware, no one has disproved the conclusion reached by Mr. Winwood Reade, that the so-called "gorillas" were really dog-faced baboons.

Similarly we notice on page 352 a repetition of the old legend as to the harmonious relations existing between the prairie-dogs and the burrowing owls (*Speotito*), whereas Dr. Elliot Coues, and after him Captain Bendire, have expressly stated that the owls certainly prey on the young, and probably sometimes on the adults, of the "dogs." We believe, moreover, that the idea of flying frogs (p. 103) has been proved to be a myth. Then, again, we are told that the European beaver never makes dams, although it did so in the 13th century. Has the author, we may ask, any reason to discredit Meyernick's account of the dam-building beavers near Magdeburg in 1829?

In another passage (p. 116) the author observes that when "we descend to the Lias and Trias and Carboniferous rocks we come upon a great variety of" Labyrinthodonts. Now, although one of these creatures occurs in the Lower Jurassic of Russia, we are unacquainted with any Liassic representatives of the order, so that it is, in any case, incorrect to speak of Labyrinthodonts as occurring "in great variety" when we reach that formation. Later on (p. 373) the author does injustice to himself in stating that chevrotains—without any reference to the fact of the generic distinction of the African and Asiatic forms—are common to the Ethiopian and Indian regions; while, when giving hunting-leopards as distinctive of the latter region alone, he is incorrect.

Most of the illustrations appear to have been specially executed for the work, but we regret that we cannot congratulate the artist on the result of his efforts, none of the figures being good from an artistic point of view, while some—especially those of the seals—are positively bad. It would, moreover, have been better had the artist paid a little more attention to the relative sizes of the animals he depicts. For instance, it is decidedly misleading to find the figure of "the smallest anteater," on p. 256, far larger than that of "the great anteater," two pages back; and a similar remark will apply to the proportions of the koala, on p. 49, compared with those of the thylacine, on p. 47.

While commending the book as a readable and instructive one to those desirous of gaining some knowledge of vertebrate zoology, apart from technical details, we cannot but regret the presence of the errors alluded to above, which, we fear, will be accepted as gospel when coming from the pen of a zoologist of the high standing of Professor Mivart. R. L.

A History and Description of the Modern Dogs (Sporting Division) of Great Britain and Ireland. By R. B. Lee, 8vo. Pp. xiv. and 584. Illustrated. London: Horace Cox, 1893.

Till the appearance of Darwin's classic works on this subject, the study of the various groups of domestic animals, and the extraordinary modifications which have been produced in them by careful breeding,

were far too much neglected by zoologists; but at the present day these narrow views have been, to a great xtent, abrogated, although there are still some naturalists who affect to despise the subject. To those, however, who, like ourselves, are of opinion that a full a cquaintance with domestic animals is essential to a right understanding of evolution, the appearance of the present volume will be welcome as showing to what extent our modern dogs are still being modified under the selective care of the breeder. The work—which treats only of the breeds more or less closely connected with sport—is, of course, written for the breeder and sportsman rather than the naturalist, but the latter will be able to select the portions of special interest to himself and skip the remainder.

An especial feature of the book is the excellence of the illustrations, which are in the form of plates by one of the photo-engraving processes, and are far superior to those in any other English work of similar size. As a rule, no particular dog has had his portrait taken for the group he represents, but a "generalised" dog is in most cases depicted, and in this the author has been decidedly well-advised.

Mr. Lee's remarks on the scenting power of the bloodhound—in the course of which various old stories are disposed of—will be found of special interest, while as an instance of remarkable modifications produced of late years, the reader may refer to the account of the modern English breed of the dachshund. Following the practice of English shows, the author considers the great Dane as inseparable from the German boarhound; but if he will turn to the dogs depicted under these names in the third edition of Brehm's "Thierleben," he will find a considerable difference between them; and this is surely a subject in which the views of foreigners ought to receive a large amount of weight. We are glad to see that the author endorses the view that one of the so-called Irish wolfhounds was a boarhound.

The work ought long to remain the authority on the subject of British sporting dogs, and we hope that it may ere long be followed by a companion volume from the same pen on the other groups.

R. L.

THE FOOD OF PLANTS: An Introduction to Agricultural Chemistry. By A. P. Laurie, M.A., B.Sc. Pott 8vo. Pp. ix. and 77. With 15 Illustrations. London: Macmillan & Co., 1893. Price 1s.

A NEAT little primer, "written as an experimental introduction to Agricultural Chemistry for beginners," and embodying a number of simple experiments by which the principles of plant nourishment are put before the student.

The idea of the book is an excellent one, theory and practice being happily combined, but occasionally the author's statements are somewhat crude or even misleading, while a sentence ending with a preposition and the use of "and which" are not to be commended.

In Chapter I., the relation between the plant and water is well and simply illustrated, but it is wrong to speak of the young root as "covered with little branch roots or *hairs*," branch roots are one thing and hairs another, and to make the two synonymous is to inculcate error for some one else to eradicate. Again, we question the wisdom of teaching that the soil holds "the water just as a sponge does"; when one of the points to be impressed on the embryo

agriculturist is that the interstices of the soil must be filled with air, and not saturated with water like a sponge.

Chapter II. deals with food obtained by the plant from soil, and is excellent as far as it goes, the student being taught that soluble substances only can pass into the roots, and that there are certain substances in the soil, soluble in water, which are absorbed by the roots for food, but no reference is made to the greatly increased power of solution afforded by the acid reaction of the root-hairs, or the carbonic acid gas dissolved in rain-water.

The nature of the soil, the "substances of a leaf," the composition of the air, and the absorption of carbonaceous food therefrom, are well explained, though surely in the heading on p. 40, "The Seed obtains Food from the Air," we should read Seedling for Seed. In the seventh and last chapter, on the source of the nitrogen required by plants, the author is a little too crude and brief in his explanation of nitrification and the tubercles on the roots of leguminous plants, "full of minute creatures, to which the name of bacteria have (*sic*) been given." The two appendices "Notes on the Experiments," and "On the use of the balance," contain useful hints.

Mr. Laurie should have got a botanical friend to look over his proofs, and thus have avoided the serious errors which mar what might have been a capital little book.

PRÉCIS DE TÉRATOLOGIE, ANOMALIES ET MONSTRUOSITÉS CHEZ L'HOMME ET CHEZ LES ANIMAUX. Par L. Guinard, Chef des Travaux de Physiologie a l'Ecole vétérinaire de Lyon. Précedé d'une préface par M. le Dr. Camille Dareste. Pp. 512. With 272 figures. Paris: J. B. Baillière et Fils, 1893. Price 8 francs.

AFTER the great work of Is. Geoffroy-Saint-Hilaire, published from 1832–1837, for many years there was little done in Teratology. A few isolated papers contained descriptions of unusual monsters, but the attention of naturalists was so absorbed by the new light thrown on the domain of normal Nature that her freaks and sports were almost disregarded. Then, in quite recent years, came Camille Dareste's series of studies on the artificial production of monsters, and a number of other studies of which Mr. J. A. Thomson gave an interesting *résumé* in the last number of NATURAL SCIENCE.[1] The more recent part of the study of monsters concerns itself with the conditions of their production, and has been called by Dareste Teratogeny, as distinguished from Teratology, the record and classification of the types. The present volume is limited designedly to Teratology, and is meant to be a compact and convenient guide to the known types of abnormality; for, if it be, at the first glance, a paradox to write of types of abnormality, or to discuss the laws of disorder, a very brief study of M. Guinard's volume will convince that monsters are not the frolic of chance, but an inverted or tip-tilted regularity. Some are caused by mere mechanical pressure or torsion, and cause and effect tread on each other's heels as closely as the pressure of a boot and the growth of a corn; in others unusual condition, raising or lowering the temperature beyond the eurythermal limits, alteration of the natural position, conditions of strain (as on a centifrugal machine) tend to produce abnormalities, although the nexus of cause and effect is invisible; but, however produced, the productions fall readily into categories. M. Guinard gives, in a series of chapters, accounts of simple anomalies, classified according to the organs or

[1] NATURAL SCIENCE, vol. ii., pp. 294–306.

tissues chiefly affected; of complex anomalies, like hermaphroditism, apparent or real; and of monsters single or double. The illustrations are very useful, and as appropriately horrible as the subject demands. There is an index; full table of contents; and glossary of terms.

Altogether it is a most useful book, and is to be commended to all anatomists and embryologists. P. C. M.

THE EMBRYOLOGY AND METAMORPHOSIS OF THE MACROURA. By W. K. Brooks, Professor of Animal Morphology in the Johns Hopkins University, and F. H. Herrick, Professor of Biology in Adelbert College. National Academy of Sciences, vol. v., Fourth Memoir. Pp. 325–576, with 57 Plates.

IN this beautiful memoir the authors have made a notable contribution to knowledge of the development of the Macroura. Animals which, like the Crustacea, pass through a series of successive moults during their larval life, naturally exhibit a series of gradations leading towards the adult condition. Fritz Müller and Claus showed that in this group a very large number of orders, families, genera, and species display the relation between ontogeny and phylogeny. At the same time, Claus pointed out that an equally large number of adaptive larvæ are to be found, and in the introduction to this memoir Dr. Brooks strongly insists on the necessity of careful and exhaustive study in each case before the significance of a larval history can be understood. The survival of larvæ living free in water depends on their adaptation to their present environment, and so recapitulation of ancestral stages in the present metamorphosis must depend to a large extent on the persistency of those external conditions to which the larvæ were originally adapted.

The chief forms studied in this memoir are *Stenopus hispidus*, *Alpheus*, and *Gonodactylus chiragra*. In addition to anatomical and embryological detail, a number of side issues of general interest turn up. Thus one would expect (says Dr. Brooks) that the least specialised species are the most widely diffused. But the larvæ of *Stenopus hispidus*, although *Stenopus* is one of the most highly specialised of the Crustacea, are most widely distributed. Specimens from the Indian Ocean and the South Pacific agree with those from the West Indies down to the most minute marking. "It is well protected from enemies by a thorny armour of hooked spines which cover all the upper surface of its body and limbs, and, as all the hooks point forward, the attempt of an enemy to swallow a *Stenopus* must be difficult and painful."

Many of the species of *Alpheus* live as parasites in the canal of sponges, and individuals of the same species living in different species of sponges may differ greatly in colour and habits.

In *Alpheus* and in *Stenopus*, after impregnation takes place, the nucleus divides and a syncytium of eight nuclei is found. In *Alpheus* there is a slight invagination and a modified gastrula is found. But in all the accounts of development given readers will be struck by the want of distinctness in the separation and formation of the germinal layers. It certainly seems as if the familiar successive origins of the germinal layers are present only in a very vague manner in this group.

The account of the larvæ of *Gonodactylus chiragra* is specially valuable, as so little is known of the Stomatopoda. In the present work, Dr. Brooks is able to confirm most of the results he reached in the "Challenger" monograph.

CONTRIBUTIONS TO THE ANATOMY OF THE ANTHROPOID APES. By Frank E. Beddard, M.A., Prosector to the Zoological Society of London. *Trans. Zool. Soc.*, vol. xiii., part v., 1893 (pp. 177–218, plates xx.–xxviii.).

THERE will be few people who failed to hear at least of the existence of the celebrated chimpanzee "Sally"; her reputation, like that of some others, higher, perhaps, in the zoological scale, was made not entirely by her own unaided merits, but was largely due to the assistance of the Press. Her lamented death gave rise to obituary notices more extensive than those commonly devoted to archbishops. Human greatness, however, is apt to be but transitory; and apparently also the same is the case with anthropoid apes; but the waning reputation of "Sally" will, perhaps, be renewed by the above-quoted memoir upon her anatomy. "Sally" belonged to a species of Chimpanzee which was originally described by M. du Chaillu as *Troglodytes calvus*; the species was not by any means universally accepted at the time of its description; but the present paper sets at rest any doubts which may be still entertained upon the matter. The question of the species of Chimpanzees is one which is not yet decided, nor is it yet ripe for settlement; the material in the way of skulls and skeletons available in the museums of Europe is not sufficient. A great many different names have been at various times applied to supposed species; but at the present moment no more than two can be regarded as having been definitely proved; those are, of course, *Troglodytes niger* and the species which forms the subject of Mr. Beddard's memoir. This ape was remarkable not only for being the only individual of her kind ever exhibited at the Regent's Park menagerie, but also for her intelligence and long life. From the condition of the teeth it appears that she was about eleven years old; at any rate she lived in the Gardens for no less a period than eight years, which is, for an anthropoid, quite phenomenal. "Sally" finally yielded to a complication of diseases which rendered it impossible to give any account of the viscera; this is to be regretted, as the chances of studying the visceral anatomy of this species are not likely to be frequent. The absence of any account of the internal structure of the animal is, however, made up for by an account of the muscular anatomy and of the brain. The latter organ is well figured in several aspects; as might be expected, it does not show any great differences from that of the common Chimpanzee; nor is the muscular anatomy particularly characteristic; such points of difference as exist are dwelt upon by the author. The reader may be interested to hear that the brain is now to be seen in the Oxford University Museum; the skin has been stuffed and is in the Museum of the Hon. Walter Rothschild at Tring. A great feature of the paper, as is, indeed, usually the case with the excellent publications of the Zoological Society, is the illustrations; we do not intend to imply by this that the text in any way falls short of the illustrations; but it certainly is the case that the plates with which the publications of this Society are adorned are of their kind unsurpassed. We have in the present paper figures life-size of the full face, of the head seen from above (evidently to show its dolichocephalic character), of the hands and feet, and a whole plate and part of another is devoted to the brain. The second part of Mr. Beddard's paper deals with another anthropoid, who was familiarly known by the name "George"; this animal was thought to be a representative of the smaller Orang, *Simia morio*; Mr. Beddard, however, is not convinced of the correctness of this view, which was partly based upon the supposition (erroneous, as it now turns out) that the ape was elderly;

"George" appeared on an examination of the teeth to be after all a mere baby. A beautiful coloured lithograph of the head occupies one plate, and the hands and feet are also illustrated. It is, perhaps, unnecessary to say that the memoir is entirely technical in character, though the first few pages, occupied by some discussion of the species of Chimpanzees, will be found to be of somewhat more general interest.

THE BIRDS OF DERBYSHIRE. By F. B. Whitlock, with Notes by A. S. Hutchinson. 8vo. Pp. vi. and 235. Illustrated. London and Derby: Bemrose & Sons, 1893.

THE fact that no complete history of the bird-life of such an important and interesting county as Derby has hitherto appeared, affords ample justification for the issue of the little volume before us, in which the subject appears to be as fully treated as materials permit. The author calls attention, however, to the lack of local observers in the wilder districts of the country; and it may be hoped that the result of his labours will be to stir up other ornithologists to complete our knowledge of the subject. To show how necessary is local observation, it may be mentioned that, according to the author, till quite recently the merlin was considered to be only a casual winter visitor to the county, whereas it actually breeds on the high peak. It is to be regretted, however, that the efforts of the pestilent gamekeeper have, within the last year or two, almost, if not entirely, exterminated this falcon from its Derbyshire haunts. Of the five plates illustrating the work, four are views of scenery, while the fifth represents a remarkable variety of the corncrake.

Although the work is, of course, to a great extent of local interest, it contains many observations bearing on the subject of British ornithology in general, and must therefore be of value to all students of that science. R. L.

FOREST TITHES AND OTHER STUDIES FROM NATURE. By "A Son of the Marshes." 12mo. Pp. 208. London: Smith, Elder & Co., 1893.

THE tendency of modern science to become a mere record of "section-cutting" and of the dry details of comparative anatomy is so marked, that we have sometimes feared whether the old-fashioned field-naturalist was not in as much danger of extermination as many of the animals in which he took such a delight. Happily, however, the observer who writes under the *nom de plume* of "A Son of the Marshes" shows that the field-naturalist still exists among us in his best form; and his observations on the varied types of animal life that may still be seen within easy access of London affords us another example of the truth of the old story of "Eyes and no Eyes." Most of the essays in the little volume before us have already seen the light as separate articles, either in the *Times*, the *Cornhill*, or *Blackwood*, where many of our readers have, doubtless, ere this, perused them with pleasure. All who have done so, we think we may safely assert, will have still greater gratification in seeing them in their present guise, as it would have been a thousand pities had such delightful reading remained buried in the pages of a magazine or the columns of a newspaper. Whether in describing the otter, as he slinks alone the river-bank, with which his coat harmonises so closely in colour as to render him invisible to unpractised eyes, or in recording the movements of the

heron watching for trout at some broken weir, the author is equally at home ; and we can thoroughly recommend his book to all lovers of Nature as a living picture of many British animals in their native haunts. R. L.

The Recrudescence of Leprosy and its Causation. By William Tebb. 8vo. Pp. 412. London: Swan Sonnenschein, 1893. Price 6s.

This is a book written to show that vaccination is accountable for the spread of leprosy. We go so far with the author in entirely condemning the system of arm-to-arm vaccination, for it is definitely certain that though the health of the child may be satisfactory, in 99 cases out of 100 there is no means of ascertaining the condition of its progenitors. The author has collected together much information concerning leprosy that is valuable to the professional as well as interesting to the general reader, and concludes by observing that, as the disease is "practically incurable, it behoves all interested in the public well-being to do their best to prevent its diffusion," which he considers is largely due to the practice of vaccination.

Evolution and Man's Place in Nature. By Henry Calderwood, LL.D., F.R.S.E., Professor of Moral Philosophy, University of Edinburgh. Pp. 349. London: Macmillan & Co., 1893. Price 7s. 6d.

In the minds of many some of the value of a book on Man's Place in Nature, written by the holder of an endowed chair of Moral Philosophy in a Scotch University, will be discounted from the outset. The Professor must hold a brief for his client, and his client is Man as a Moral Agent. The interest of the book or the lectures for such readers resolves itself into a curious contemplation of the byeways by which the author shall arrive at the known goal. The Scotch Professor of Moral Philosophy, at whose feet this reviewer had the duty of sitting, cleared a Stanley path through the forest of Science, raucously scaring the poor pigmies, shooting down the detestable niggers, hacking through the forest, marching straight with blundering and boisterous declamation on his inspired mission. Quite other is Professor Calderwood. On his way to his conclusions he dallies with variation and environment, embryology and evolution, very agreeably passes the time of day with Huxley and Hæckel, Eimer and Weismann, Helmholtz and Darwin, and says what very intelligent fellows (in a moderate and non-moral way) they all are. But when the real business comes on (in the last chapter) the naturalists rather fade away, and are replaced by Aristotle and Plato, Butler and Green, Shakespeare and Holy Writ, and Professor Ray Lankester ; for Professor Ray Lankester has written on "Degeneration"—a branch of the theory of Evolution held in high honour among the dogmatic.

Professor Calderwood accepts the principle of evolution so far as man's body is concerned. With the proviso that life itself is inexplicable on mechanical or chemical grounds, and with the usual quotation from Huxley against abiogenesis, readily enough he concedes that there is an enormous preponderance of evidence in favour of the organism of man being of the same kind, and descended in the same way, as other organisms ; but in addition to this organic life, he claims for man "rational life" unexplained by science and giving to man a dominance and position in nature totally unex-

plained by his physical nature. To arrive at this, he goes over a large field, where it would be impossible to follow him in detail, pointing out a multitude of distinctions—as, for instance, that man is a producer, animals only consumers, animals are modified by environment, man can modify environment, animals have sensibility, man rationality, and so forth. All these are points of discussion that, however Professor Calderwood trick them out in new settings, have been debated abundantly; and it is no unfair thing to say that one knows beforehand how the facts will appear to different men. Professor Calderwood and his kind exaggerate the mechanical rigidity of "instinct," dilate on the breaking down of animals in unaccustomed circumstances, and dwell on the glories of Shakespeare and Newton. The Darwinian and his kind dwell on the gradations by which "instinct" seems to pass into deliberated adaptation of means to ends, lavish adulation on the intelligence of the dog, and grovel before the animal stupidity of the savage.

Professor Calderwood, and with him many others, still fear what Carlyle called "the monkey damnification of mankind." They have lost the old fears now that they have lost the battle over the evolution of the body: they redouble them on the question of reason. To this reviewer it seems inexplicable that as all men know we have (to use the popular terms) reason, conscience, power of making moral judgments, and the apparent choice between them, it should matter whether these came suddenly or gradually; and it appears extraordinary that when all the processes of the world (including evolution) are a "sovereign wonder of superhuman fixedness of law," any writers should be so concerned to establish jerkiness in the law. Thus, Professor Calderwood with considerable pride, leads up to an argument for the existence of deity by coming to the conclusion that he has shown the incompetence of science to explain life, mind, and reason. Then he goes on: "There is a Power operating continually in Nature, which does not come within range of the observation possible for scientific modes and appliances, yet to which science is ever indirectly bearing witness. This Power has manifested itself at the most impressive periods in the world's history, first at the appearance of Organic Life, again on the appearance of Mind, and again on the advent of Rational Life." A smoothly-running process, an organic growth of the world of matter and life, a growth on which the fruits of life, of mind, and of soul appear in their due season, seems to Professor Calderwood less compatible with theology than an interrupted and repeatedly tinkered Nature.

This book is devoid of scientific value, and no doubt was not intended to have any; and while it cannot conceivably help to support the faith of anyone who has not in his mind a thought-tight partition between faith and reason, it may be a consolation to many good stupid people sorely tried in their faith.

THE YEAR BOOK OF SCIENCE. Edited for 1892 by Professor T. G. Bonney, D.Sc., LL.D., F.R.S. 8vo. Pp. 519. London: Cassell & Co., 1893. Price 7s. 6d.

IN this second volume of the Year Book of Science the unevenness of the different sections is still very noticeable, and the book might be greatly improved by stricter editing. We observe that the authors of the different parts take various views as to the objects of

this annual. One section or chapter may be all that it ought to be, and may give a good outline of the year's advance in some branch of science; but the next section is treated quite differently, and leaves us in entire ignorance of the advances made, except in some small field specially cultivated by the author.

As examples of good outlines we may mention the Mineralogy and Petrology, by G. T. Prior; Stratigraphical Geology, by H. B. Woodward; Palæontology (Vertebrate), by R. Lydekker; Palæontology (Invertebrate), by J. W. Gregory. Then comes a section on Palæobotany, by T. Hick, in which the whole of the prolific literature published during the year on fossil plants is ignored, except a few species from the Coal-measures, and a single Alga from the Permian. To this author the whole Tertiary and Secondary flora is non-existent!

The sections on Physical Geology and Geography, by H. G. Seeley; and Anthropology, by H. G. Seeley, are very imperfect. Biology (Animal), divided into several sections, and treated of by C. H. Fowler, C. S. Sherrington, R. I. Pocock, and R. Lydekker is much better.

Biology (Botanical) is divided between W. B. Hemsley, who takes Systematic and Geographical Botany; G. Massee, who writes Morphology and Biology of Plants; D. H. Scott, who treats of the Minute Anatomy of Plants; and F. E. Weiss, who abstracts the literature on the Physiology of Plants. But in some unaccountable way all these botanical writers have forgotten to mention Lubbock's large monograph on Seedlings! Each probably thought that it would be in another section, and so it was missed altogether.

M. Martinus Nijhoff, of the Hague, has just issued a "Catalogue de Livres sur les Possessions Néerlandaises aux Indes Orientales et Occidentales sur l'Empire Indo-Britannique, les Possessions Espagnoles, Françaises, Portugaises, la Chine et le Japon, l'Australie." The catalogue is arranged under headings, and will be of assistance to those interested in these countries. Pp. 280.

The Administrator's *Annual Report of British New Guinea from 1st July, 1891, to 30th June, 1892*, has just been issued as a Blue-book by the Colony of Queensland. The affairs of the Possession are reviewed under the separate heads of Legislation, Administration of Justice, Administrative Visits of Inspection, Government Property and Works, Establishments, Meteorology, Trade, Mission Work, Lands, Prisons, Finance, Native Dialects, Scientific Contributions, and Reports by Officers. It contains 113 pages of text and 9 maps. No one can read this official document without being impressed with the extraordinary amount of information it contains of the highest scientific value. This young colony is more favoured than most in having an officer in charge who is not only a highly efficient Administrator, keenly alive to all the best interests of the natives, but a man possessed of an unusually wide range of attainments. Under the head of Administrative Visits of Inspection, the dispatches contain an amount of geographical, geological, zoological, anthropological, and linguistic data, collected chiefly by the Administrator himself, of unusually high value. The Report contains also sub-reports by officers specially commissioned for the purpose, *i.e.*, by Mr. Moreton on his unsuccessful Expedition from Philipps Harbour towards

Mount Suckling; on Meteorology, by Mr. Clement Wragge, Government Meteorologist of Queensland, and a long and valuable report on the Geology of the Possession by Mr. Gibb Maitland, of the Queensland Geological Survey.

Meteorological observations are taken at four stations in the Possession, at the capital, at Mekeo, and at the headquarters of the Eastern and Western districts. "Unfortunately, complete records can hardly be procured, except at Port Moresby, owing to the fact that officers at other stations are often necessarily absent from their headquarters on other duties." At Port Moresby the mean atmospheric pressure for the year 1891, at 9 a.m., was 29·919 inches. The grand mean temperature was 83·0 F.—the highest shade (2nd February) 96·2°, the lowest (27th August) 72°. The rainfall for the year was 72 inches, of which 53 inches fell in the first five months of the year. It rained on 95 days. At Samarai (in China Straits), from April, 1891, to March, 1892, 126·5 inches fell on 174 days. During the three months of July, August, and September a fall of 59 inches was recorded there, while only 15 inches fell at Port Moresby. Dufaure Island is the meeting place of the Port Moresby and Samarai climates. Sir W. Macgregor contributes, as an appendix, a vocabulary of the Kiriwina dialect in the Trobriand Islands, with observations on its grammar, and the Rev. W. Bromilow one on the aboriginal vocabulary of Dobu.

In concluding his Report, which, we repeat, is of great scientific value, the Administrator remarks: "The results of native administration appear, on the whole, fairly satisfactory for the year. In not a few districts natives are settling down to more systematic work in preparing exports for the trader. . . . In some of the districts, where native murders were fearfully common two or three years ago, this crime has completely disappeared, or become very rare. No doubt what in these days may be called 'much time' is required to acquaint the Papuan tribes with the working of the Government, and the impartial and beneficial action of law. But they possess an aptitude for such tuition which cannot but appear to be extraordinary to any person acquainted with the history of civilisation among the present existing cultured races of our globe. If the Papuans are allowed anything like reasonable time—a time which compared to that required for such a purpose by other races would be a very short period—there can be no doubt that they will become a very considerable, if not an important, unit in the Australasian dominions of the Queen." These observations strikingly confirm the opinion of the natives formed by Mr. H. O. Forbes during his intercourse with them, even before Government supervision had begun to take effect, and expressed in *Blackwood's Magazine* for July, 1892.

The English Catalogue of Books for 1892 has just appeared (Sampson Low, five shillings). We are glad to see that the plan of arrangement adopted last year is adhered to; there is no system more convenient for a reference book than the purely alphabetical. We would suggest, however, one improvement, which is that all the miscellaneous collection at present under "Transactions" should fall into their proper alphabetical position in the next issue. "Transactions" is clearly wrong, "Serials" or "Academies" if you will, but not "Transactions."

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Mr. John Storrie has resigned the Curatorship of the Cardiff Museum.

Dr. Jordan has arrived at Tring from Hildesheim, and will take charge of the Entomological collections belonging to the Hon. Walter Rothschild.

Mr. George Massee has been appointed to succeed Dr. M. C. Cooke in the Kew Herbarium. The second volume of his *British Fungus Flora*, dealing with Agaricineæ, has appeared.

Mr. F. A. Bather, of the British Museum of Natural History, has obtained special leave of absence on account of a weakness of his eyesight. Mr. Bather will spend the greater part of four months on the sea and will visit Teneriffe, Cape Town, Hobart Town, Sydney, Japan, San Francisco, Burlington, and New York. He left on March 30.

Mr. A. Smith Woodward is on his way back from the Lebanon, where he had gone to study the important deposits of fossil fish of Cretaceous age.

The funds of the McGill University have been increased in the sum of 160,000 dols., the gift of Mr. Molson and Mr. Donald Smith.

The syllabus of work for the "Edinburgh Summer Meeting" for the seventh Session, July 31—August 26, 1893, has just been issued. The business of Section C (Natural Science) consists of ten lectures on Comparative Psychology, by Professor Lloyd Morgan; Hygiene, ten lectures by Dr. Louis Irvine; Biology, twenty lectures by J. Arthur Thomsom and Norman Wyld; Practical Botany (including Field Work), twenty meetings, Robert Turnbull; Field Geology, ten excursions directed by Norman Wyld; Practical Zoology (at Granton Marine Station), twenty meetings, J. Arthur Thomson; Regional Survey of Edinburgh and neighbourhood, twelve lectures.

There will also be delivered twenty lectures on Contemporary Social Evolution by Professor Geddes, and many other courses on various branches of Education, such as History, Social Science, Music, Elocution, &c. Particulars can be obtained by application to J. Arthur Thomson, M.A., University Hall, Edinburgh.

WE are glad to observe that a suitable inscription has been recently placed on the pedestal of the bust of Sir Richard Owen, which has a place of honour in the Pavilion of the Palæontological Gallery at the Natural History Museum. This bust is a plaster cast of the Hamo Thornycroft marble (1880), preserved at Sheen Lodge, and was presented to the Museum by the family about four years ago. The inscription reads as follows:—

SIR RICHARD OWEN,
K.C.B., M.D., D.C.L., LL.D., F.R.S., &c., &c.
Superintendent of the
Natural History Departments
of the British Museum,
from 1856—1883.

Born, 20th July, 1804. Died, 18th Dec., 1892.

"Accesserunt ossa ad ossa, unumquodque ad juncturam suam."—
Ezech., xxxvii., 7.

We are informed by Dr. Henry Woodward that the appropriate quotation from the Vulgate was selected by the Rev. Professor Bonney.

THE fourth annual meeting of the Museums' Association, which is to be held in London under the Presidency of Sir W. H. Flower, K.C.B., will commence on Monday, July 3, and probably extend over four or five days. The mornings will be devoted to the reading and discussion of papers, but the afternoons will be given up to the inspection of various metropolitan museums, under competent leadership.

A GEOLOGICAL Society has recently been organised in Washington for the presentation and discussion of topics of interest to geologists. The constitution and standing rules were subscribed to by 109 founders at the first public meeting, March 8, 1893. Its members are of two classes, active and corresponding. The annual subscriptions of the first are 2 dols., and of the second 1 dol. Meetings will be held on the second, and generally also on the fourth Wednesday of each month, from October to May inclusive. The journals and bulletins of the various societies appear to furnish sufficient opportunity for the publication of papers read before the Society, so that for the present the Society will not undertake to publish the papers presented. It will probably issue one bulletin each year containing the address of the retiring President, and such other matter as the Council directs. The officers, elected February 25, 1893, are C. D. Walcott (President), S. F. Emmons and W. H. Holmes (Vice-Presidents), A. Hague (Treasurer), Whitman Cross and J. S. Diller (Secretaries); the Council being G. F. Becker, T. M. Chatard, G. H. Eldridge, G. K. Gilbert, G. P. Merrill. We should like to have seen the organisation of local excursions among their objects, for field observation is an essential to the increase and diffusion of geological knowledge.

THE collection of Lepidoptera (chiefly Micro-Lepidoptera) formed by the late H. T. Stainton has been presented by his widow to the Natural History Museum. The presentation is the more valuable as it includes the original drawings and papers illustrative of the specimens.

THE Zoological Society of London has just issued its Annual Report of Accounts for 1892. We are glad to note a slight improvement in gate-money (£272) over 1891, which points to an increased appreciation in the efforts made by the Society to render the Gardens interesting to the public. It is curious to see a falling-off in the elephant rides of £3 19s. 4d., but still the comfortable amount of

£606 was received from this source during the year. There is a considerable improvement in the admission fees, which points to an encouraging influx of new Fellows. Turning to the payments, we find that out of an expenditure of £25,968 no less than £17,618 was expended on the Gardens, an amount which speaks eloquently for the excellent standard maintained. Of this amount, £843 was spent on animals and their transport, £3,974 on food, £3,468 on salaries and pensions, £765 on horticulture and garden-work, £2,628 on works and buildings, and £3,466 on menagerie expenses. We have commented on the expense of the *Zoological Record* on page 324.

Mr. Edward Best, who has been on the staff of the Geological Survey for 38 years, has now retired. His place as acting secretary is taken by Mr. W. Topley, and Mr. A. C. G. Cameron has been promoted to the rank of geologist.

At the annual meeting of the Norfolk and Norwich Naturalists' Society, held at the Norwich Museum on March 28, Mr. Thomas Southwell was elected president. The address of the outgoing president, Mr. H. B. Woodward, was devoted to various local topics, principally geological.

The work of remounting the great series of microscopic preparations made by the late Professor de Bary, and acquired by the British Museum, is nearly completed. The slides illustrating the comparative anatomy of the vascular plants, and those dealing with the structure and life-history of the lower forms, have been successfully restored, and the Fungi, which have been left to the last, are three-parts done. The value to students of Botany of the presence of this great collection in this country can only be indicated, since it will always remain as the authentic basis of the great botanist's work.

Professor Herdman sends us the "Sixth Annual Report of the Liverpool Marine Biology Committee." The laboratory at Port Erin appears to be a flourishing institution, and a considerable amount of work has already been turned out. The Committee have secured the services of Mr. Henry Vanstone, of the Royal College of Science, South Kensington, as "resident Curator," and important additions have been made during the winter to the laboratory. A great number of students appear, from what we have heard, to be availing themselves of the opportunities of working there.

We regret to learn that Mr. T. D. A. Cockerell has found the climate of Kingston, Jamaica, too trying for his health, and has been obliged to leave the Museum there and return to his old home near the Rocky Mountains. Mr. Cockerell was doing some good work in economic entomology, and his departure will be a great disappointment to him and a distinct loss to the Kingston Museum. His address will be Las Cruces, New Mexico, U.S.A.

Mr. E. J. Bles has been appointed Director of the Laboratory at the Plymouth Marine Biological Station.

We hear that Professor Albert Gaudry has resigned the position of Professor of Palæontology at the Jardin des Plantes.

OBITUARY.

ALPHONSE LOUIS PIERRE PYRAMUS DE CANDOLLE.

Born October 28, 1806. Died April 4, 1893.

THE great Swiss botanist died at Geneva, in the old house in the Cour St. Pierre, which, built by one of two brothers who took refuge in Geneva at the Reformation, had been the home of the family for many generations.

He was born in Paris a few days after his father had been rejected on his second presentation to the Institute in favour of another botanist, much his inferior, Palissot de Beauvois, and in his autobiography published by his son in 1862 ("Mémoirs et Souvenirs"), Auguste speaks of the consolation he derived from the happy event. Two years after, Auguste went to Montpellier, where he succeeded Broussonet as Professor of Botany, but in 1816, owing to political troubles then rife in France, he resolved to return to his native town, at the University of which a chair in natural history was founded expressly for him. This he held till 1834, when he resigned, his place being taken by his son, Alphonse, the subject of the present memoir.

The name of De Candolle would always be an honoured one among botanists, even if Auguste had not had in Alphonse a son to carry on his great work and strike out new lines of his own, while, now the son is gone, Casimir, the grandson, a well-known botanist, lives to perpetuate its fame in the third generation.

Every botanist who has any knowledge of systematic botany, every gardener who lays any claim to scientific knowledge, knows and has proved the grand *Prodromus Systematis Naturalis Regni Vegetabilis* which, begun in 1824 by Auguste and finished in 1873 by Alphonse, will be a lasting memorial of their genius and perseverance. There was grit in these old botanists.

Nowadays when a man scores a paper in a few weeks or months, or perhaps the best part of a year, one looks back with admiration at Auguste de Candolle, no longer young, starting, in 1816, to monograph all the Orders of the vegetable kingdom according to the then but little known natural method. When, after five years of hard work, only eleven Natural Orders were complete, including, nevertheless, two of the most important—Ranunculaceæ and Cruciferæ—he

realised that the work was too vast, and started on the *Prodromus*, evidently, from its name, intended as an abridged form of the larger work. After the first few volumes, however, the *Prodromus* became itself a series of complete monographs, thus ultimately realising Auguste's original ideal. The first volume appeared in 1824, and the name of the father alone appears on the title-page of this and the six that follow. In his historical account in the seventeenth and last volume, Alphonse says his father was almost entirely responsible for the matter of the first seven volumes, at which he worked unceasingly for twenty years, from 1822 till his death in 1841. From the third volume onwards, however, he was assisted by his son, who, after his father's death, carried on the work for thirty-two years more, adding ten volumes. Though he received a great deal of help from other botanists, of whom he gives a list, with the number of pages due to each, he was solely responsible for the editing, and also worked up twenty-six of the monographs.

Folks may wonder, he says, why, with so many helpers, the work took so long. But monographers find their difficulties increased with the number of species, specimens, characters, and synonyms. Moreover, rarely are herbaria and books to be had at one and the same time without some hindrance or delay, libraries being often separated from the plants, or wanting in the more recent works. Here a most devoted botanist lacks either the books or plants, while there the botanist is himself wanting in zeal, method, or time for work. There are also authors who, having undertaken a monograph, do not forward it in due time, or even give it up altogether. This last failing in particular caused loss of time, and necessitated frequent division of the volumes into sections and transposition of the Orders, one (Artocarpeæ) having, at the last moment, to be abandoned, and, finally, the conclusion of the work, at the end of the dicotyledons, "ne tertiam botanicorum generationem occideret!" In the "Monographiæ Phanerogamarum," however, the number of missing Orders is gradually being reduced, seven volumes having been issued at the time of his death, while others are in active preparation.

The *Prodromus* includes 214 natural orders, 5,134 genera, and 58,975 species; 15 per cent. of the genera and 25 per cent. of the species are new. Of the 13,194 pages, the de Candolles (including Casimir, who supplied 260) wrote 5,950 pages, various curators of the Candollean Herbarium 1,475, and other botanists, among whom are three Englishmen—Bentham, Sir J. Hooker, and A. H. Weddell—5,769.

But the *Prodromus* was not the sole outcome of all these years. Until 1850 Alphonse occupied the Chair, instituted for his father, at the University of his native town, but in this year resigned it, in consequence of the state of political affairs in Geneva. In 1835 he had published an "Introduction to the Study of Botany," a book of two volumes, and in 1844 produced a third edition of his father's

"Théorie Elémentaire." In 1855 appeared his "Géographie botanique raisonnée," published in two volumes, and containing, in 1,300 pages, "an exposition of the principal facts and laws concerning the geographical distribution of plants at the present day." "In its aggregation of facts and results, and in their skilful marshalling," says a reviewer in the current issue of the *Gardener's Chronicle*, "it is comparable only with the works of Darwin, some of whose views were indeed something more than foreshadowed by the great Swiss botanist." In 1867 he laid before the International Congress of Botanists at Paris his "Lois de la nomenclature botanique," which still forms the basis of modern botanical nomenclature. In 1880 appeared "La Phytographie," or the art of describing plants, a work of the utmost value to every systematist, and concluding with an invaluable list of the principal herbaria. His "Histoire des Sciences et des Savants depuis deux siècles" (1873) is a charming book, and quite intelligible to the general reader, while his "Origine des Plantes Cultivées," a translation of which is published in the "International Science Series," is also well-known.

These do not represent nearly all of the stately and courteous Professor's contributions to botanical science, but this is but a short biographical notice, not a bibliography, and no place can be found for his numerous papers dealing with very various aspects of botany.

De Candolle made several memorable visits to this country. When about two-and-twenty he came over to London, and leaving the metropolis in May went down to the West of England. Thence he worked up through Wales and the English Lakes to Glasgow, where he met Sir William Hooker, then Professor of Botany at the University. From Glasgow he went to Skye, and having done the island on foot, crossed to Inverness, and so back to London, where he arrived again in the autumn. Certainly a very creditable tour, considering the means of communication more than sixty years ago.

In 1866 he presided over the London Botanical Congress held at South Kensington, and in his address emphasised the great importance of an intimate relation between botany and horticulture. In May, 1850, he was elected a foreign member of the Linnean Society, of which he was at his death the senior foreign member.

In 1889 the Society conferred on him its gold medal, the highest honour it has to give. The President, when announcing the sad news at the meeting on the evening of April 6, mentioned that his son Casimir, who has done good botanical work, had been selected by the Council for nomination as a foreign member.

De Candolle loved the English and English ways, dining in English fashion and speaking English largely at home, and, in common with the rest of his family, was by no means friendly disposed to the political powers of his native city.

He was well and vigorous till within six months of his death,

when he began to feel old. His wife died some years ago, but he leaves two sons, to one of whom, Casimir, we have already referred as a well-known botanist.

WE regret to announce the death of the Rev. T. Wolle, pastor of the Moravian Church at Bethlehem, Penn. He was the author of books on the *Fresh-water Algæ of the United States* and on the *Desmids of the United States*. His work has value as that of a systematic pioneer and recorder of forms—a labour to which he brought great zeal and enthusiasm.

PROFESSOR Giuseppe Antonio Pasquale, who died at Naples on February 14, was Professor of Botany at the University, and Director of the Botanic Gardens. He was the joint author of a "Flora medica della Provincia di Napoli," published in 1841, and a "Compendio di Botanica," which reached a third edition in 1890. He also wrote an account of the "Flora Vesuviana," and other systematic and morphological papers, and compiled a "Catalogo del real orto botanico di Napoli."

CORRESPONDENCE.

I HAVE to thank Dr. Hurst for bringing to my notice his interesting suggestion as to how inoculation may be supposed to confer immunity from disease. The idea that the attenuating process produces, by selection, a new strain of microbe having a different physiological action, seems worthy of careful examination. Such a gradual production of a more slowly-increasing variety well explains the diminishing intensity of the disease produced by successive inoculations. As Dr. Hurst points out, the diapedesis of the leucocytes is soonest able to overcome such feebly-increasing organisms. There is, however, to my mind, this difficulty. Attenuating processes consist generally in growing under *unfavourable* conditions, and I cannot see why such conditions should favour the slowly-increasing rather than the rapidly-increasing varieties of the microbe. Indeed, it seems to me probable that, other things being equal, unfavourable conditions generally would rather select the more quickly-increasing. Quick breeding is such an obvious advantage under adverse circumstances, as affording, in each generation, more numerous chances of survival. There remains, of course, the suggestion that the selection may "act in some other way."

Supposing, then, we have a virus attenuated in the manner suggested by Dr. Hurst, how can immunity conferred by inoculation with the same be explained? We may take it as an established fact that the local inflammation attending inoculation is accompanied by diapedesis of the leucocytes. These leucocytes, in their abnormal state of activity consequent on inoculation,—and, perhaps, as Dr. Hurst suggests, in increased numbers,—and, after their victory over the feebler army of microbes, might be ready to attack and subdue the more formidable foe. This would explain momentary immunity, but I cannot see how such a state of

abnormal activity—which is inflammation—can be supposed to continue for months or years. Nor does it seem to me probable that an increased rate of production of the leucocytes within the body could be rendered permanent by the temporary action of the attenuated virus; for it is to be remembered that the immunity is supposed to extend over certain prolonged intervals—nine months in splenic fever, according to Pasteur, and seven years for the small-pox, according to the medical faculty; and if protective inoculation merely consists in the increase in the strength and activity of the standing army of leucocytes, then *one* inoculation should suffice for *all diseases*; he who is protected by inoculation with the cholera virus should be proof against small-pox, and the virus of scarlet fever should protect against typhus. Moreover, might not such a state of leucocytal activity be produced by other means, without the introduction of microbes or poison from a diseased animal—an idea which, to say the least, is lacking in poetry? That such were the case is, indeed, a "consummation devoutly to be wished" and prayed for. If Dr. Hurst can prove his proposition he will have the thanks of a long-suffering public, threatened with the nightmare anticipations of a life spent in being inoculated—a veritable flying from "the ills we have to others that we know not of."

Finally, I must remind Dr. Hurst that the inoculation liquids of many experimenters contain no microbes. Dr. Koch's consumption vaccine, I believe, contained none, nor does that of Dr. Kraftkine

G. W. BULMAN.

"TWINS AND TRIPLETS" IN *Doris*.

MR. J. ARTHUR THOMSON refers to the frequency of "twins and triplets" in *Doris*, meaning thereby two or more embryos developing within a single egg, and suggests that it may be due to "the fact that the ribbons [of eggs] are battered to and fro by the surge."

I have observed the same phenomenon, however, in eggs laid by a *Doris* in an aquarium, and not exposed to any movement at all.

April 15, 1893. C. HERBERT HURST.

TO CORRESPONDENTS.

All communications for the EDITOR *to be addressed to the* EDITORIAL OFFICES, *now removed to* 5 JOHN STREET, BEDFORD ROW, LONDON, W.C.

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ERRATA.

P. 254, line 21, for "Caprettæ" read "Caprellæ."

P. 255, line 13 from bottom, for "Hasting" read "Harting."

P. 259, the block was lent by Dr. Sclater.

NATURAL SCIENCE:

A Monthly Review of Scientific Progress.

No. 16. Vol II. JUNE, 1893.

NOTES AND COMMENTS.

Fog and Vegetation.

AMONG the most interesting of last month's reports is that issued in the *Journal of the Royal Horticultural Society* (part 1, vol. xvi.) by Professor F. W. Oliver, who publishes the second instalment of his observations on the effects of urban fog on plants cultivated under glass. The effect is twofold; in the first place, we have to consider the serious loss of light; in the second, the poisonous substances present in the air.

The general tone of plants, especially of those fond of sunlight, must be considerably lowered by the often long persistent, dull winter weather, with its frequently-recurring fogs. The assimilation of carbonic acid is interfered with, while the transpiration of water vapour from the leaves is almost at a standstill. The roots are not affected, and continue to absorb water from the soil, especially in the case of stove plants, where they are kept by the warmth in a state of marked activity. Hence the cells of the plant become unduly distended with water. Finally, owing to faulty circulation in the intercellular passages, access of oxygen from without is impeded, while the supply normally received from the decomposition of carbonic acid in assimilation is almost entirely cut off; respiration is checked, and substances tend to accumulate in the cells from want of complete oxidation. Thus the whole leaf-mechanism is out of gear. In this enfeebled state the plant is exposed to the attacks of sulphurous acid, hydrocarbons, and other noisome fog constituents, and the result is lamentable, often, indeed, fatal.

Professor Oliver distinguishes two classes of injury, produced by distinct causes. First, cases in which the leaves show local discolourations, particularly at the tips and margins, while the unaffected parts remain fully functional, and the leaf does not fall; secondly,

cases in which the leaves fall, showing either a complete brown or yellow discolouration, or only a partial one limited to the apex, margins, or base, or restricted to minute specks, or irregular patches scattered over the surface. Sometimes the leaf falls uninjured; according to Mr. Watson, during some recent fogs bushels of healthy-looking leaves were gathered up almost every morning in the Palm House at Kew.

The local blotchings of the first class are presumably due to the action of an acid on the upper surface of the leaf. The layer of dirt deposited by the fog contains an appreciable amount of sulphuric acid, the product of oxidation of sulphurous acid. The frequent wetting of the leaves brings this into solution and the drops having a tendency to collect at the tips and margins will leave there on evaporation a deposit of acid. This process is continually repeated until sufficient acid accumulates to corrode the surface.

The changes in the leaf which cause its rapid fall, with or without a colour change, are, the author thinks, largely due to an attack on its delicate unprotected internal tissues. The fog effects an entry through the stomates into the system of intercellular spaces, where its poisonous ingredients come in immediate contact with the moist, delicate, and unprotected membranes of the living cells, which offer but little resistance, and the protoplasm is directly attacked. Whether the leaf succumbs or not depends on the inherent constitution of the protoplasm. The process was carefully followed in leaves of Rhododendrons and others, and the action was found to begin in the lower layers of the spongy tissue next the stomates. Thence it spread to the upper parts of the leaf and the epidermis. In the case of thin uncuticularised leaves when the epidermis is very soft, the noxious vapours may enter directly through the outer layer, as well as by the intercellular spaces. It is not clear to what component of the fog this second class of injuries is due. Owing to the very general absence in the injured leaves of acid products of the green colouring matter, sulphurous acid cannot be the sole agent, and Professor Oliver thinks that some complex organic bases like pyridine may play an important part. As regards remedial measures, there seems but little to be done, except, as Lindley long ago suggested, to keep the temperature as low as is compatible with the life of the plant, with sufficient humidity to avoid desiccation. If the houses are otherwise tolerably air-tight, a contrivance for filtering the incoming air through boxes containing trays of charcoal sticks may be used to mitigate the evil.

A Technical Education Conference.

On April 20 and 21 the Senate House at Cambridge was the scene of a Conference on the relation of Universities to the County Councils in regard to technical education.

The meeting was a representative one. Delegates from the Board of Agriculture, the Agricultural Society, the Oxford and London Universities Extensions, and numerous County Councils met a number of the lecturers of the various societies and others interested in the subject under the auspices of the Cambridge University Extension Board, with the Vice-Chancellor, Dr. Peile, as chairman.

At the first session the "needs of rural districts" and "local organisation" evoked some spirited discussion; a certain difference of opinion was manifest as to the former, while, as regards the latter, a point of vital importance, many of the lecturers had to report a serious apathy on the part of local authorities and persons of influence. Co-operation between neighbouring counties and between County and Town Councils was also the subject of a paper.

At the second session, Professor Liveing spoke on the sequence of subjects of instruction; the meeting then passed to the consideration of the Cambridge and Counties Agricultural Education Scheme. This will ensure a course of two years' instruction in subjects bearing upon Agriculture, including Chemistry—Elementary and Agricultural; Botany—Elementary and Agricultural; Physiology; Geology; Economic Entomology; Book-keeping, Mensuration and Surveying, and Agricultural Engineering. The course will occupy about half of each year, so that those intending to become farmers will have the other half in which to study the practice of farming. Certain Professors and Teachers in the University will admit to their lectures, and to practical instruction in their laboratories, students who, being over seventeen years of age, shall give satisfactory evidence of a sufficient previous education to enable them to profit by such instruction. Several County Councils have voted money for the support of the scheme, while a grant of £100 has been made by the Board of Agriculture. The same County Councils have also offered scholarships to assist promising young men desiring to take the course. Finally, it is hoped that the University will shortly sanction an examination in connection with the course and will grant a diploma to successful candidates.

We wish the scheme a hearty success, and hope that those whose future is to be devoted to farming will take advantage of means which will enable them to become intelligent farmers at a very small cost, and thus go a long way to solve the vexed questions of making agriculture more profitable.

Gigantic Australian Marsupials.

At the last meeting of the Zoological Society of London (May 16), Professor Alfred Newton communicated a letter and drawing from Professor Stirling, of Adelaide, referring to the discovery of a great accumulation of skeletons of *Diprotodon* and other extinct marsupials in South Australia. The find has already been briefly

recorded by the publication of a telegram in the *Times* of April 21, and, judging from the letter, it seems likely to complete our knowledge of the skeleton not only of *Diprotodon*, but also of other extinct Australian animals. The bones are said to occur in a salt marsh in a wonderful state of preservation; but the country is now so hot and arid that the difficulties of digging and transport are very great.

Diprotodon, it may be explained, was a wombat-shaped animal about as large as a rhinoceros—the largest marsupial hitherto discovered—and, with the exception of its feet, the skeleton is now almost completely known, thanks to the explorations of Dr. Bennett and others and the researches of Sir Richard Owen. The history of the gradual discovery of the animal is one of some interest. The name *Diprotodon* was first given by Sir Richard Owen in 1838 to the anterior end of a lower jaw obtained by Sir Thomas Mitchell in the Wellington Caves, New South Wales. Five years later, a drawing of part of a jaw with teeth reached England from the same source, and this Sir Richard Owen believed to represent a kind of *Dinotherium*, indicating for the first time the occurrence of primitive elephants in Australia. In the same year, a portion of a molar tooth, associated with the shaft of a femur and other fragmentary bones, was also received, and the same anatomist wrote: "The fossils, which my friend has now transmitted, incontestably establish the former existence of a huge proboscidian Pachyderm in the Australian continent, referable to either the genus *Mastodon* or *Dinotherium*." Only a year later, however, these early surmises proved to be incorrect, and within a short time Sir Richard Owen was able not merely to describe most features in the osteology of *Diprotodon australis*, but also to distinguish another allied genus, *Nototherium*. The feet alone remained unknown, and parts of these were described in the *Philosophical Transactions* of the Royal Society in 1886 as the toes of that fabulous monster, the "Great Horned Lizard of Australia" (*Megalania prisca*, Owen). Complete skeletons, such as Dr. Stirling leads us to expect, will no doubt help much in the determination of minor points and classificatory matters; but the researches of Owen and later authors leave little to be learned about the main features.

The Earliest Monkeys.

In a recent paper on the Eocene Mammals of North America, Messrs. Osborn and Wortman announce their belief that the European *Adapis* and certain allied American forms instead of being, as generally supposed, Lemuroids, are really monkeys. The ground for this appears to be that they have normal lower canine teeth, instead of having the first premolar modified to serve this function. We cannot, however, see that this is a valid reason for their separation from the Lemurs, the earlier forms of which, in our view, were probably ancestors of the Monkeys. This, however, we suppose, is nowadays heresy.

The Ancestors of the Cat.

The above-named authors in the same communication put forth the hypothesis that the cats, instead of being directly related with the other Carnivores of the present day, are independently descended from the Creodont genus *Palæonictis*. In view of the close similarity between the Cats and the Civets (the latter being clearly related to the Dogs), this is "parallelism" with a vengeance. Does it never strike the promoters of such extreme views that by proving (!) the want of relationship between apparently closely allied animals they may be cutting away the ground from beneath the foot of the evolutionist, since, if resemblances are, so to speak, fortuitous, why should not we revert to the doctrine of separate creations? That parallelism does exist in Nature, we are fully prepared to admit, but it is not omnipresent. However, when once the ball is set rolling in a certain direction, everyone thinks it necessary to give it another push.

Aërial Roots of Orchids.

The velamen, or characteristic external covering of the aërial roots of epiphytic orchids, forms the subject of a note by P. Groom in the last issue of the *Annals of Botany*. As terrestrial orchids are almost invariably devoid of a velamen, it is usually assumed that the aërial orchids acquired theirs subsequently to the adoption of an epiphytic mode of life, but it is suggested that the few exceptions may indicate the existence of the covering in a previous terrestrial mode of life.

With a view to getting some light on the date of origin of the velamen, the author has made observations on *Grammatophyllum speciosum* and species of *Bromheadia*, both at Singapore. The former is one of the few orchids which grow naturally both as terrestrial and epiphytic plants, and can be found growing in the jungle when it has happened to fall off a tree. When a terrestrial plant, the velamen is not only retained but even strongly developed in the soil, while the upwardly-growing aërial roots characteristic of the epiphytic habit, which Schimper has shown to be respiratory organs, are still developed.

Bromheadia alticola lives in the full blaze of the sun on high tree tops, and has a peculiar two-layered velamen, while in *B. palustris*, which is terrestrial, the velamen is fundamentally similar, but lasts only for a short time, peeling off and disintegrating. This points to the conclusion that this covering is essentially adapted to epiphytic plants, and was evolved subsequently to their assumption of an aërial mode of life.

The two sets of observations seem to give contradictory results. In the first, the velamen being more highly developed in the subterranean; in the second, in the aërial roots; but the explanation is found in the fact that the function of the structure varies. In *Grammatophyllum* it is essentially an absorbent organ, which not only

persists, but is more highly developed, while the root becomes subterranean ; whereas in *Bromheadia* it is mainly protective, preventing loss of water, while the absorptive function is carried on by root-hairs borne on the lower surface, which, in the terrestrial species, are reduced to strongly cuticularised papillæ.

Hence, in the aërial branches of the subterranean roots of *Grammatophyllum* the velamen dwindles, while in the subterranean roots of *Bromheadia* it peels off so as not to interfere with the process of absorption.

Science and Dictionaries.

The explanation of the sins of omission and commission in scientific words in dictionaries would task the acutest mind, and it is very dismal to notice that Dr. Murray's monumental English dictionary is not conspicuously better than its less ambitious predecessors. In an idle hour we dipped into the " A's " and brought up the following :—

Sins of omission—

Alecithal : Amphiblastula : Analogy (in its scientific opposition to " homology ") : Asteroidea : Anamniota : Anthropomorpha : Acœlous or Acœla.

Sins of commission—

Ascidian—A group of animals belonging to the Tunicate Mollusca : considered by evolutionists to constitute a link in the development of the Vertebrata.

Anthozoa—Another name for the zoophites called Actinozoa.

The Imperial Institute.

Fellows of the chartered scientific societies have received so many pressing invitations to become Fellows of the Imperial Institute that they have long been anxiously awaiting a definite statement of the privileges and work in which they might share. Since the opening by the Queen, on May 10, their wish has at last been gratified by the appearance of an advertisement, which is strongly suggestive of that of the Crystal Palace and the Earl's Court show, and seems to explain that the Institute is to be another summer resort for Londoners. A music and dancing licence has been obtained, and on four days a week the public are to be admitted to the entertainments at popular prices. " Tea, coffee, and other refreshments will be served from the garden kiosks. . . . Seats and tables may be reserved in the public dining-rooms by making application to the dining-room superintendent. . . . The full band of the Royal Artillery will play daily in the kiosk of the West Garden. . . . Entertainments of vocal and instrumental music will occasionally be given in the Great Hall and the Indian Pavilion." Fellows and their friends have the sole enjoyment of these privileges on Wednesdays and Fridays.

Cholera.

The May number of the *New Review* has a sensible and readable article by Dr. Robson Roose on the propagation and prevention of cholera. Numerous cases are cited in illustration of the intimate relation between the water supply and cholera outbreaks, and hence the supreme importance of the purity of water used for drinking. The author does well in pointing out that dirty filters are worse than none at all; unless it is from time to time cleansed or renewed, fairly good water may actually take up impurities from the filter. If charcoal be the agent it should be boiled occasionally, say once a month, and then dried in the sun or an oven. Spongy iron filters are recommended for general use as being cheap and easily renewed. The fact that water is cool and sparkling does not imply purity. An outbreak in Golden Square in 1854 was traced to a well, the water from which was much liked for having these characteristics, but on examination was found to be contaminated by leakage and filtration from a cesspool. The general rules for prevention of the epidemic are those of ordinary hygiene, cleanliness in all things, moderation, and care in diet and exercise. The fact that more than five-and-twenty years have passed since cholera gained a footing in this country, though it has from time to time reached our ports, may fairly be attributed to the improved sanitary conditions which now obtain in all our large towns.

A study of the range of the Molluscan genus *Placostylus* has lately led Mr. C. Hedley to generalise in reference to the ancient geography of the region of New Caledonia, the Solomon Isles, New Hebrides, and Fiji Isles, and their connection with New Zealand (*Proc. Linnean Soc. N. S. Wales*, ser. 2, vol. vii., 1893, pp. 335–339). He thinks that these islands form part of a shattered continent, never connected with, or populated from, Australia, but rather deriving their fauna from Papua *viâ* New Britain. The presence of genera common to Australia and New Zealand is explicable on the ground that they migrated, not from the one territory to the other, but each from a common source, New Guinea. New Zealand and New Caledonia seem to have been early separated from the northern archipelagoes and to have ceased to receive overland immigrants therefrom. Finally, the Fijis appear to have remained to a later date in communication with the Solomons, though severed from that group before the latter had acquired from Papua much of its present fauna.

The problem of stocking a pond with fish receives an unexpectedly bold solution in our contemporary *Illustrated Scientific Facts* (April 15); it is there said that, as the bottom of many ponds consists of soil which was once the bottom of the sea, it probably abounds in spawn deposited, say, by the fishes of the Old Red

Sandstone; the vivifying influence of the water hatches out the spawn, and hence the appearance of fish. The paragraph is gravely worded, and there is not the least indication that it is "writ sarcastic."

IN the *Trans. Entom. Soc. Lond.*, 1893, pp. 191–9, Dr. D. Sharp describes the very remarkable eggs of a Reduviid bug from the Amazon valley. The eggs were closely arranged on a leaf, and a wasp, supposed to have been killed by the mother bug to furnish food for her young, was entangled in the mass by the wings. Each egg is cylindrical, the upper part containing a conical, crown-shaped structure, composed of a system of network and tubes believed to afford entrance to spermatozoa. This cone is pushed upwards by the young bug in emerging, and the upper part of the egg-capsule, which has kept it in place, is ruptured. Numerous parasitic Hymenoptera were bred from the two outer rows of eggs, the cones of which had not been lifted. It seems that the habit of laying the eggs in a serried mass secures the safety of the majority within by the sacrifice of these outermost rows, beyond which the ovipositor of the ichneumon-fly cannot penetrate.

AN interesting paper on the Pupæ of Moths, by Dr. T. A. Chapman, appears in the latest number of the *Trans. Entom. Soc. Lond.* (1893, pp. 97–119). He distinguishes two principal types:—the obtected pupa, which has a hard, even surface, with the skin of the appendages closely attached to that of the body, and with the fifth and sixth abdominal segments alone capable of movement; and the incomplete pupa in which the appendages are more or less free, maxillary palpi are present, and three, four, or five abdominal segments can be moved. The obtected pupa, also, has no power of progression, but the incomplete generally emerges from its cocoon before transformation into the moth. The division of the moths by means of these pupal characters roughly corresponds with the old division into Macro- and Micro-lepidoptera, but the Pyraloids, having obtected pupæ, are classed with the higher moths, while the Zygænidæ, Sesiidæ, Cossidæ, Hepialidæ, and Limacodidæ are placed with the Tineids, Tortricids, and Micropterygids. Most of these changes have been suggested by naturalists working mainly by the neuration of the wings; the confirmation afforded by this distinct line of research will hasten the general acceptance of the new views.

AT the meeting of the Entomological Society of London on March 8, Dr. Sharp read a paper on Stridulating Ants. He said that examination revealed the existence in ants of the most perfect stridulating or sound-producing organs yet discovered in insects. We have only received the abstract of this paper, and must wait fuller details before commenting on it.

Several contributions have lately been made to knowledge of the distribution of the mollusca. Mr. A. Everett has made extensive collections of land-shells in Borneo and the Philippine Islands, and these have been described by Mr. E. A. Smith (*Journ. Linn. Soc. Zool.*, vol. xxiv., 1893, pp. 341–352, pl. xxv., and *Ann. Mag. Nat. Hist.* [6], vol. xi., 1893, pp. 347–353, pl. xviii.). Mr. A. Abercrombie has collected 320 species of mollusca on the coast of Bombay, and 25 are determined to be new by Mr. Cosmo Melvill (*Mem. and Proc. Manchester Lit. and Phil. Soc.*, vol. vii., 1893, pp. 17–66, pl. i.). A list of 205 species from the Seychelles is also given by P. Dautzenberg (*Bull. Soc. Zool. France*, vol. xviii., 1893, pp. 78–84).

The *American Naturalist* for April contains an article, by Professor E. D. Cope, on the Genealogy of Man. Professor Cope agrees with M. Topinard in believing that the Hominidæ descended directly from the lemurs, without the intervention of the Simiidæ. A special feature of this article is a plate showing the peculiar character of the grinding faces in the teeth of the Palæolithic men of Spy.

The vexed question of Man and the Glacial Period is discussed over twenty pages of the *American Geologist* for March. Messrs. Shaler, Wright, Leverett, Upham, Claypole, Winchell, Hitchcock, and Putnam contribute to the discussion, which has apparently been induced by the publication of Professor Wright's book, noticed in this Journal for February. There are also some papers on Glacial deposits; and those on Pleistocene Geology, read at the Ottawa meeting of the Geological Society of America, held in December, 1892, also appear in print.

One result of the recent excursion of the Geologists' Association to Norfolk (to which we called attention in our April issue) was the finding in the Norwich Crag, at Bramerton, of a portion of an antler of *Cervus sedgwicki* ?, Falconer. The specimen was obtained by Mr. R. W. Hinton, and identified by Mr. E. T. Newton. Although named with a query, it belongs to a form hitherto not recognised out of the Cromer Forest Bed.

The subject of chlorophyll in animals has just been discussed again in a lengthy article by Dr. E. L. Bouvier (*Bull. Soc. Philom. Paris* [8], vol. v., 1893, pp. 72–149). He admits that the green colouring matter is sometimes diffuse in Infusoria, but he considers that in the large majority of cases there is distinct proof that it occurs solely in symbiotic algæ of the family Palmellaceæ. He points out that these green specks are frequently found free in the water, and in that case they multiply by zoospores like the isolated algæ of certain lichens. He has also observed the inoculation of organisms by these free chlorophyll-bearing cells.

A RECENT number of the *Farmers' Bulletin* (no. 10), issued by the United States Department of Agriculture, describes the Russian Thistle, *Salsola kali*, the Prickly Glasswort of our sandy sea-shores, as one of the worst weeds ever introduced into American wheat fields. One year's damage in Dakota alone is estimated at 2,000,000 dollars. The bulletin reports that it takes complete possession of the soil, while its spiny nature makes it objectionable to horses and other animals.

IN *Le Naturaliste* of March 15 Henri Coupin argues that zygomorphy of the flower, that is, symmetry in relation to a single plane, is primarily for the purpose of protecting the pollen from rain, while adaptation for pollination by insects is only a secondary consideration.

THE "Dictionnaire Pratique d'Horticulture" is an improved translation of Nicholson's well-known Dictionary of Horticulture. It is edited by M. S. Mottet and is, says the *Gardeners' Chronicle* of May 6, "indispensable to all who read French, as it is more complete than the original edition, and contains several additional features." It is issued in parts by Octave Doin of Paris.

THE *Journal of Botany* has been showing a tendency towards Cryptogamic Botany during recent years. In the April number, there is a paper on Fresh-water Algæ, one on Marine Algæ, one on a Moss, one on Hepaticæ, and a long obituary notice of a Cryptogamic Botanist. The editor probably means no more by this than that Cryptogamists (even though one fewer) are getting too many for him.

MR. HERBERT SPENCER has issued a reprint of his articles on "The Inadequacy of Natural Selection," followed by that on "Professor Weismann's Theories," appearing in the *Contemporary Review* for February, March, and May, 1893. The pamphlet is published by Messrs. Williams and Norgate.

PROFESSOR LOGAN LOBLEY has in the press "The Parishes of Surrey: their Surface Features, Geological Structure, and Natural Resources." The book will deal with the 148 parishes in the county, which will be taken in alphabetical order. It is proposed to treat the subject in as practical a way as possible, and to render the volume a thoroughly useful compilation.

We have had the opportunity of seeing a specimen copy of the first part of Mr. Lydekker's "Royal Natural History," and are glad to be able to report on the splendid style in which this important work is got up, the illustrations looking far better when set up among English type than they do in Brehm's ponderous tomes among German letterpress. If this part be not in the hands of our readers by the time they peruse these lines, we believe it will be issued in the course of the current month.

Mr. J. W. Gregory has decided, with characteristic energy, to push on through the Mount Kenia country to Lake Barengo, and proposes to return by the unfrequented Sabaki route. The district is fairly healthy, there are many geological and zoological problems awaiting solution, and we trust that he will be successful in doing good work; he may thus save the reputation of the Villiers Expedition, so seriously injured by its originator. Sir Gerald Portal's cautiously worded reference to Lieut. Villiers was quoted in our April number.

The Government of India has decided to dispense with the services of natives as Geological Surveyors. The reason for this decision is stated to be, that habits of observation and practical enquiry are not sufficiently developed in the Hindoos by the present system of education. We also understand that the Government has issued further orders restricting the work of the Surveyors to questions of economic interest.

We have received the *Bulletin of the Geological Institution of the University of Upsala*, vol. i., no. 1, edited by Professor Hj. Sjögren. This is a new serial, and is intended only for papers worked out at the Geological Institution of the University, or based on material belonging to the collections of the Museum. At present, it is proposed to issue a yearly number, which will contain a report of the meetings of the geological section of the Upsala Students' Association of Science, in addition to the original articles. Subjects may be treated in French, German, or English. The present number opens with "Contributions to Swedish Mineralogy," by the editor, in which Axinite, Hedyphane, Humite, Chondrodite, Clinohumite, Longbanite, Szabite, and Adelite are treated, the last three being recently-discovered forms. C. Winran writes "Ueber das Silurgebiet des Bottnischen Meeres"; O. Nordenskjöld on "Der Hälleflinten des nordöstlischen Smålands"; and J. G. Andersson on "The Occurrence of the Paradoxides ölandicus-zone in Nerike." The *Bulletin* is illustrated with five plates of minerals.

I.

Flowers in the Guiana Forest.

THE "spicy breezes" of the tropics are proverbial, and many travellers have written on the beautiful flowers and their perfumes. According to the popular notion, these odours are perceptible at considerable distances from the coasts, and greet the weary mariner almost before he sights land. This, however, if not altogether untrue, is highly exaggerated. When there is a land breeze, it more generally brings with it a heavy stifling odour redolent of the mangrove swamp and decaying vegetation, no doubt recalling memories of the forest, but rarely pleasant, and, sometimes, even disagreeable. On the rivers, where a heavy mist hangs at night, perfumes are sometimes wafted to considerable distances, but rarely can they be detected at sea, probably because there is always some motion in the air which disperses the vapour.

The ordinary visitor, with exaggerated notions of the beauty of tropical flowers, expects to see them here, there, and everywhere, and is naturally disappointed when he finds so many trees and plants with, what he considers, no blossoms at all. Coming from meadows dotted with moon-daisies or golden with buttercups, he sees only a few inconspicuous weeds, with here and there a yellow rattle-bush (*Crotalaria*), or the blue *Ruellia tuberosa*. In the gardens, however, he can hardly fail to be struck by the size and beauty of the numerous species of *Hibiscus*, *Convolvulus*, and *Bignonia*, but these are not wild flowers.

The true native plants are neither to be found in field nor garden in the cultivated districts. To see them he must paddle along the rivers and creeks, walk on the sand-reefs, and explore the savannahs; but, even here, he will be disappointed if he expects to see anything like a continuous stretch of colour such as gratifies the eye on an English down or mountain slope. The flowers in the tropics do not grow in great masses, like heather, furze, or broom, but one species here and another there, rarely in anything like a clump, or even scattered in any considerable number. Then they do not all open at the same time, but some are earlier and some later, even in the same species. Again, few remain open for any lengthened period; some can only be seen by the early riser, as they close soon after sunrise,

while others unfold with the dawn and droop and wither in an hour or two.

Wallace, in his "Tropical Nature," heads a section, "Comparative Scarcity of Flowers," and states that "conspicuous masses of showy flowers are so rare, that weeks and months may be passed without observing a single flowering plant worthy of special admiration." This is such a sweeping assertion, and so contrary to the facts, that, notwithstanding the author's status as an authority on the tropics, we can hardly do other than try to refute it.

In the dark arches of the forest scarcely a flower is to be seen; but even here we often come upon a litter of golden or crimson petals which have fallen from above, so that, although we cannot see that the tree is flowering overhead, we know it must be a magnificent spectacle if we could only rise above it. Along the banks of the rivers and creeks there are always flowers, sometimes few, sometimes many, but at no time entirely wanting. Most of these are borne on creeping and scrambling vines, some are particularly showy, and even an ordinary observer must admit that many of them are worthy of special admiration. Again, there is the sand-reef where the low bushes, at certain seasons, are decked with flowers, which can be easily seen, and are by no means rare.

One of the reasons why so few flowers are noticed is because they are not looked for at the right time. It cannot be expected that forest trees will be in flower all the year round, although they may blossom twice instead of once in twelve months. In Guiana the flowering seasons are in February and March, and July and August. During these periods masses of showy flowers are common, although many of them are so far above our heads, or so hidden by the forest canopy, that we can only get an imperfect glimpse of them.

The expanse of green foliage is so great as almost to overpower all but the most gaudy flowers. Whites and all dull colours are obliterated, and even yellows combine with the sunlit green in such a manner as to be invisible at a distance, unless, as in the case of the etabally (*Vochysia*), the whole tree is covered with flowers. The large waxy-white flowers of the clusias and yellow and cream Malvaceæ, are quite invisible, while the great spikes of Mimoseæ can hardly be seen from the ground unless specially looked for. Rarely does a tree become bare at the time of flowering, and even when this takes place the individual is almost lost in the great crowd. The foliage is so dense that we can only compare the expanse to a roof. When Wallace states that from some elevated point you often gaze down upon an unbroken expanse without a single patch of bright colour, he speaks the truth, but it does not follow that there are no showy flowers there. Take away the background of foliage, if this were possible, and many a brilliant group of flowers would appear. Even in an English orchard the difference between those trees which flower before and those with the leaves is enormous.

A great many forest-trees have greenish-white or green flowers. These are, of course, very inconspicuous at all times, but they by no means eclipse those with showy flowers. Rarely do even two or three of a species come together, so that there will be always some of the more conspicuous even in a small area. When we mention that the predominant natural order is the Leguminosæ, it will be seen at once that there can be no scarcity of flowers. This family is a host in itself, the various forms of Papilionaceæ, Cæsalpineæ, and Mimoseæ all combining to make an interesting and showy collection. Then there are the monkey pots (Lecythideæ), all of which have large and highly-coloured blossoms; but the most conspicuous tree is, undoubtedly, the etabally (*Vochysia*), which, during the season, is a canopy of gold, the result of flocks of sulphur-coloured butterflies.

It is unnecessary to give a list of the different families which make up the forest flora. They are so numerous and varied, so different from anything in the European woods, as to be most striking to the ordinary observer, and a continual source of interest to the botanist. It is, however, their beauty of foliage which compels attention at first; the shapes and sizes of their leaves, their luxuriance, and their continual struggle to occupy every little patch of sunlight to the exclusion of all others, which prevents the forest from ever becoming dull or monotonous.

Book illustrations rarely give any adequate representation of forest scenery. If they are not conventional they are generally almost caricatures. Artists have attempted to picture some of the most striking peculiarities, but even they only observe the superficial. Appun's illustrations in "Unter den Tropen" (Jena, 1871) are among the best, and as nearly true to life as a draughtsman can make them; but in the whole series there is hardly a flower, this proving that the artist was impressed by the foliage almost to the exclusion of everything else.

In paddling up a creek the canopy of forest-trees is so far overhead as to be out of the line of sight. Every bend brings into view a new scene. Clumps of graceful palms, masses of gigantic creepers, jungles of tree-ferns intermingled with the contrasted foliage of marantas and heliconias, and great aroids which climb almost every tree or perch on their branches, all combine to leave an impression of rampant vegetation which eclipses the most gaudy assemblage of flowers.

But, with all this, flowers are not wanting. Great bunches of yellow bignonias, dipladenias, allamandas, scarlet noranteas, combretums and cacoucias, with spikes which glow like fire in the sunlight, a hundred species, of various shades, from rosy crimson to purple; and last, but by no means least, the tubular Cinchonaceæ and other white flowering plants, the clusters of which often show up quite prominently against the dark background of foliage. Some of those with lurid flowers are by no means inconspicuous. The

species of *Mucuna*, which hang their flowers on long strings, one of our commonest timber trees; the wallaba, with similar blossoms; the *Marcgravia*, with its curious pitchers; and several bushes and trees with their flowers sessile on stems and branches, all go to add to the interest of forest scenery.

As if this were not enough, many of the shrubs and lower-growing plants have coloured bracts. Almost but not quite hidden by their handsome foliage, the heliconias glow with a fire-like brilliancy, and some of the marantas, although not so conspicuous, still show up well among their great leaves. A species of *Cephaëlis* has large scarlet bracts, and other plants of the same family come to the front in a similar way. Tillandsias, which are epiphytal, and have inconspicuous flowers, often shine up on the edge of the forest owing to their scarlet bracts.

Colour is by no means wanting in the leaves. The great family of Melastomaceæ, so characteristic of the American tropics, stands out most prominently as an example of beauty in form, venation, and colour. Many of the species have a crimson glow over their whole surface which marks them out from their dark-green neighbours. Their flowers, although not generally showy, are always elegant, and their dark purple fruit very pretty indeed. The young seedlings of forest trees have always two or three of their upper leaves glowing with those delicate shades which are so conspicuous at the change of seasons, and low-growing marantas are often striped with rosy or white lines. Large patches of ferns and selaginellas also cover the ground on the edge of the forest, and with their lighter tints contrast with the dark greens and browns.

Only in the dense forest, where perpetual twilight reigns, are the tree-trunks quite bare. Along the river banks, borders of the savannahs, and sand-reefs, every stem is more or less clothed with creepers. Some, as *Vanilla* and *Marcgravia*, climb like ivy, their leaves lying almost flat, others festoon the tree with garlands, while the immense heart- and arrow-shaped leaves of the giant aroids congregate at the top, almost covering their host. Other great plants of the last-mentioned family sit on the upper branches and throw down long aërial roots, the size of thick twine, which branch into masses of fibres as they reach the water. Mosses are not so plentiful as in temperate climates: they are replaced by patches of ferns, some of which are particularly delicate. Species of *Trichomanes* often extend up the trunks for several feet, where they are replaced by *Polypodium piloselloides*, and higher yet by *Peperomia nummularifolia*. These form cushions on which small epiphytal orchids, ferns, tillandsias, and gesnerias find congenial habitats, all combining to decorate the otherwise bare stems and branches.

To return to the flowers. Away from the rivers and open spaces, they are almost invisible, and even when seen many of them are inconspicuous; but, although hidden from sight, they make their

presence known by their perfumes. They may be of that greenish white which is hardly distinguishable from the background of foliage, and yet be able to attract myriads of nocturnal insects. Pass by at any time in the day and not the faintest trace of perfume lingers upon them, but paddle along at night and you wonder whence comes the overpowering fragrance.

Diurnal flowers and insects are comparatively scarce when contrasted with those of the night. The great tubular blossoms which glow in the sunlight are visited by humming-birds, butterflies, and bees, but the hum and whirr of insect life, so characteristic of night in the forest, is almost wanting. Where a few butterflies may be seen fluttering lazily from flower to flower at noon, after sunset moths are attracted in numbers by the great white blossoms, or the fragrance of thousands of smaller and less conspicuous flowers.

How difficult it is to trace these perfumes. Sometimes it is quite impossible. Who recognises the odour of the mora[1] flowers, or knows anything of the numerous scents that are wafted across the narrow rivers after nightfall? As certainly as we see a particular kind of whitish flower, we can confidently state that it is odoriferous at night, but hardly a single species is known in this way. In passing along the creek, a perfume is suddenly wafted across, but where it comes from is a problem not easily solved. Even that common orchid, *Epidendrum nocturnum*, which derives its specific name from the fact that it distils its perfume after nightfall, is rarely known by this character. We have seen cases where these orchids have been kept for years without their owners knowing anything of their perfume, and when it was discovered, this came about by the accident of bringing a plant into the house for an evening. Other orchids are quite as peculiar in this respect; a most delicious odour may be perceived at one particular time, and not be appreciable at any other.

Some flowers open for an hour or two, and then, whether fertilised or not, droop and wither; others are able to try again perhaps at the same time next morning or evening. Those which do not fall may still have their particular time for fertilisation, when it appears as if every effort is strained to attract the particular insect whose agency is so urgently required. It is surprising to find that so many are successful in these efforts. Even when their odour is not appreciable to us, the bees find them out. They may be hidden away under a tangle of bush-ropes, and not a single insect of the species required be visible in the neighbourhood, but somehow or other, as the flowers open, the bees or moths appear in considerable numbers.

Flowers must be peculiarly sensitive at these times. Some, as

[1] A gigantic timber tree, belonging to the family Leguminosæ, and forming extensive forests in British Guiana. It grows from 130 to 150 feet high, and produces a very tough and close-grained wood, which is imported into this country in considerable quantities for ship-building, even rivalling oak in its non-liability to splinter. The bark is astringent, and useful for tanning.—ED.

we know, have their temperature raised several degrees, and, probably, all are more or less heated for the short period when they are prepared for fertilisation. Several species of *Hibiscus* go through a series of changes of colour from morning to evening; the *Victoria Regia* opens the first night as a waxy white flower, and the second with rosy petals. Several orchids also change colour very quickly, especially after their work is accomplished. In a few hours the plump waxy petals become limp, the markings spread and get diffused over the lip, and the whole flower becomes unsightly. Where there are male and female flowers the latter often lasts much longer, and may even be perceptible for months at the apex of the seed-vessel.

What an important crisis in the life of the plant is its flowering time. All its energies appear to be directed to this one end and aim. We can hardly look upon it as merely vegetating, but as an individual straining to accomplish the greatest work of its lifetime. There are so many little things to be considered that, whatever our ideas of Natural Selection and the chance survival of the fittest may be, we are bound to think of them as sentient beings. When a man fixes an hour for performing certain work, and that is the most suitable and perhaps the only possible time for accomplishing it, he does little more than the orchid, which, apparently, knowing that its friend the insect will be on the wing from midnight to dawn, opens its flowers and circulates its perfumed invitations through the neighbourhood only during those few hours.

We say, perhaps, that these actions are instinctive, but what is instinct? Is it not the accumulated experience of all past ages? Every individual, whether animal or plant, may be considered as a link in a long chain of beings, the origin of which goes back to some past age when the common ancestor was nothing more than a simple cell. During this long period, in which so many generations have passed away, what a wealth of experience must have been gained. Is this lost at the death of the individual? On the contrary, do we not see that every organism has the stamp of its parentage, and we can hardly be wrong in saying that not the slightest impression has been made on any individual that is not perpetuated in its offspring. These impressions may remain latent for generations, but nevertheless be ready to come to the front at any time according to circumstances.

Suppose we compare these to a composite photograph and call them physical memories, is it not necessary that there should be some power of selection? Is everything done mechanically as reflex action? Can we not conceive that there may also be physical reason, which works unconsciously, but at the same time always towards certain well-defined ends? When we go to sleep at night with the intention of waking at a certain hour in the morning, and do so perhaps to the minute, physical memory and physical reason

has perhaps kept the record for us and done it well. That something of the same kind is at work in every plant and animal is so obvious that it can hardly be disputed.

It is utterly impossible to conceive of such curious and wonderful contrivances as are found in the orchids, especially *Coryanthes* and *Catasetum*, originating without a purpose, and being perpetuated only by the chances of Natural Selection. Given physical memory and physical reason, we see at once that the difficulty is reduced to almost nothing. For ages environment has been at work moulding every part of the plant as it were, until it has become what we see it to-day; but it cannot remain on the same level; it must either advance or go backwards. Here individuality comes in. Why does every plant and animal differ from every other? With the same inherited characters, and grown under circumstances apparently identical, two seeds from one capsule produce trees differing more or less from each other. Natural Selection may account for a great deal, but it can hardly explain the origin of variation. Taking into account, however, the fact that flowers are extremely sensitive at the period of fertilisation, is it not possible that impressions may be made on the plastic germ-cells which affect the new individual? The seeds in one capsule may not be all impregnated at exactly the same time, and circumstances may occur in an hour, or even a few minutes, which affect the one individual, then at the most critical stage of its existence, to the exclusion of others in different stages.

Many insects and plants die soon after impregnation, the males almost immediately, and the females as soon as the germs of their offspring are sufficiently mature. It follows, therefore, that the end and aim of their whole life is to bring forth new individuals. Plants can be increased by budding and other than sexual connection, but it is doubtful whether the portions so divided are equally strong with those raised from seed. They are not individuals to the same degree as if raised from two parents with different experiences. Then, again, they have not passed through that critical period when the germ-cell was highly susceptible to every external influence.

Looking upon the trees of the forest as individuals, how interesting they appear. Sometimes we may fancy them conscious, and, instead of feeling lonely in their midst, rather think that we are in good company. No longer vegetating, they live, strive to do their duty, look out for "number one," and aim at perfection. The great scrambling creepers seem to know how to get ahead at the expense of others, growing longer or shorter according to circumstances, sometimes turning to raise themselves to great heights, or at others forming dense bushes. One species has succeeded by some peculiar contrivance, and its neighbour by means entirely different. They twine round or adhere to the tree-trunks as long as it suits their convenience, and then stretch forth on every side, or perhaps hang in long streamers almost to the ground. The mangrove extends its long

slender arms downward, and branches into fingers as it nears the water, feeling its way, as it were, to the most congenial places. Near it is the courida, which anchors itself in the mud in a different manner and has given up using props. But there at intervals up the trunk are large excrescences covered with aborted aerial roots which seem to indicate similar possibilities in this tree also.

Other examples are continually forcing themselves on the attention of the naturalist, all going to prove that plants are something more than creatures of circumstance. After making every possible allowance for the influence of light, heat, and moisture, there still remains the apparent selective power, which only seems to differ in degree from that possessed by animals. Without the power of locomotion, they yet extend their roots to great distances, feeling around, as it were, for congenial food, and if they fail in one direction try several others. Then their branches are condensed at the apex, or spread out from the base upwards, according to the situation, the same species, in different places, appearing quite distinct in shape and size.

But where do we find such examples of apparent selection of means to particular ends as in the orchid family? When certain species provide quarters for a garrison of carnivorous ants, it is easy to say that the insects found the bunch of aerial roots suitable for a habitation, and that the plant did nothing. But when we see that only a few orchids have developed contrivances to this end, and the great advantage they derive from them, we cannot but look upon them as designed for the purpose. In some cases they are so well placed as not only to defy every larva and cockroach, but even man himself. In our own experience, we have had to pass on and leave a fine plant, because we could not risk the bites of such a host as swarmed out at the least touch.

Some flowers, as we know, secrete nectar for the express purpose of feeding the insects they attract by their colours and perfumes. Others attract, but provide nothing, yet the blundering insect still fertilises it in his search for what is not there. When boys play similar tricks, we call them practical jokers. Has the plant done this either accidentally or mechanically? Some go so far as to provide traps by which insects are caught and utilised; are these contrivances also due to chance? In the *Coryanthes* we have a marvellous collection of means to a particular end, *i.e.*, the fertilisation of its flower. By means of its perfume and colour it attracts bees, only to give them a bath from which they can escape by doing its work. If a man does something of a similar kind we consider him rather clever, although, perhaps, not very honest. It is now generally conceded that animals are reasonable beings; to go a step farther, and allow the same to plants, is almost as necessary. It does not follow that because a dumb man cannot explain his thoughts that, therefore, he has none. When the strong man bears pain without flinching, is it

because he feels less than the baby who cries out at the top of his voice? It is a matter of common observation that plants show their likes and dislikes, suffer and languish when sick, brighten up and look robust in congenial positions, and have, to a certain extent, most of the faculties common to man and the lower animals. True, they cannot see nor hear, but their sense of feeling is very acute, and we may conclude that even taste is not wanting.

JAMES RODWAY.

II.

Biological Theories.

V.—SUGGESTIONS AS TO THE TRUE FUNCTIONS OF "TENTACULOCYSTS," "OTOCYSTS," AND "AUDITORY SACS."

SUCH a jelly-fish as Aurelia has eight minute finger-like bodies lodged in little pouches in the edge of the umbrella or swimming-bell. The tip of each is weighted by a calcareous deposit within it, and the stalk is hollow, the cavity of the gastro-vascular system extending into it. To the whole organ, that is, to the diminutive tentacle and its pouch together, the name "tentaculocyst" is applied, and the function performed by the eight such organs is a subject which I propose to consider first.

The large nerve-supply has led to the belief that the tentaculocyst is a sense-organ, and it is not infrequently described as a combined visual and auditory organ. That it is not auditory, I have already shown. That it is sensitive to light is a reasonable conclusion from the presence of pigment, but that does not explain its structure. A simple pigment spot without pouch or tentacle would serve that purpose quite as well. There is, moreover, a wide difference between an organ sensitive to light and a visual organ, for "sensitive to light" does not necessarily mean giving rise to a sensation when light falls upon it. It may be that light produces a stimulus when it falls on a pigmented patch, but unless that stimulus gives rise to a sensation of some kind or other, the organ should not be called visual. A reflex muscular contraction may follow upon the stimulation by means of light, but that by no means shows that sensation has intervened. Little as we know of the psychology of these lowly-organised animals, we are justified in doubting, if not in denying, the possibility of consciousness and, therefore, of sensation. It is nearly impossible to believe that consciousness, and judgment, and intelligent action upon the basis of knowledge received by means of sense-organs is possible in an animal with a nervous system so lowly-organised as that of a jelly-fish, especially of a Scyphomedusa, and an explanation of the function of these organs which is free from any such assumptions as these would, therefore, seem to be called for.

If, however, these were auditory organs, as has been supposed by

many, the fact that there were eight of them would be difficult to explain, though, of course, it might be suggested that this was necessary to enable the animal to determine the direction in which the sound reached it; but, even then, the limited powers of locomotion would seem to render such a sense useless, and hence the evolution of the organs could not be explained by Natural Selection, nor yet, since the animal has no vocal organs, by sexual selection.

Temperature may, perhaps, affect them, but if so surely it would affect all alike, and eight would not, therefore, be more useful than one or two; nor is their structure explicable on the hypothesis of sensitiveness to temperature.

It may be well, therefore, to enquire what are the conditions under which the animal lives, and what are the dangers to which it is exposed.

The danger from living enemies may be left out of account, for the abundant supply of thread-cells and the very small percentage of matter useful for food which they contain make them almost useless as food for other animals, and, even if it were not so, they have no means of escaping from any animal which might pursue them.

These animals are pelagic, that is, they live in the open ocean, near its surface. The tissues of the body are so delicate that it is difficult to lift one from the water without tearing it to pieces. The dangers of the storm-tossed surface of the ocean for such an animal are, in part at least, obvious, but it is less obvious how they are avoided, especially if the animals be devoid of judgment and even of consciousness. The view I propose to offer is, that the tentaculocysts, without giving rise to sensation, serve to automatically steer the animal in such way as to keep it out of the way of its chief dangers and, at the same time, in the region where its food most abounds. How this is effected can only be made clear when we know something about the nature of the disturbance which we call a wave.

It is not easy to know very much about a wave without also knowing more about certain branches of higher mathematics than is usual among zoologists. Parenthetically, I may, as a biologist, admit that the Senior Wrangler who defined a biologist as "a man who cannot solve a mathematical problem," came sufficiently near the truth to make the epigram a distinctly unpleasant one!

Those biologists who are undismayed by formidable formulæ, and who have not as yet studied wave-movements of water from the mathematical point of view, will find the greater part of the problem treated mathematically by Lamb (1) and Basset (2); but I will assume that the Senior Wrangler was right, and that the biologist is a man of observation rather than of mathematical investigation, and I will, therefore, appeal rather to his power of observing than to his mathematical faculty.

It is unfortunate that at times in our dredging excursions, when the waves are largest and best fitted for study, those of us who lean

over the side of the boat are usually not in the best possible condition for making scientific observations. The end of a pier has, however, some advantages over a boat under way; but in a boat at anchor is the place where the observations can best be made. The best time for the purpose is when the waves are large, and especially when their crests are sharp-edged and not rounded, and when there is no wind and no breakers or "white horses." Most of the points, however, may be made out from the pier-head, though in that case the path of the particle or of the mass is not a circle, as in deep water, but an ellipse with the long axis horizontal.

A *small* floating body almost or completely submerged is not carried along with the waves, but moves when on the crest of a wave more slowly than the wave, but in the same direction. So soon as it has fallen behind the crest into the trough it moves in the opposite direction as if, having slipped off one crest, it were hurrying to climb up to the summit of the next one. In the open sea the extent of this to-and-fro movement is equal to that of the up-and-down movement, so that the body—provided it be *very* small—constantly travels in a circular orbit, the diameter of which is equal to the height of the crests above the troughs. If there be no wind it will hardly progress at all, but will return always to almost the same place. The very slight progress of particles near the surface may be neglected, and the path of each particle may be regarded as a circle.

Let us now suppose the water divided into a series of layers, which, if the water were at rest, would each have a uniform thickness of, say, one inch. The passage of waves would not cause the water of one layer to mix with that of others; but since the water of the superficial layer in a trough is moving in the opposite direction to that in the crest behind it, it follows that the length of the portion between these two points, measured in the direction in which the waves are travelling, is getting less, and this portion of the layer must therefore be getting thicker, while the portion on the back of the wave is getting thinner. The total amount of the elevation of the crest is the net result, the sum, of the thickenings of all the successive layers of water, from the surface down to the bottom. The depression in the trough is similarly the net result of the thinning of all the layers below this region.

Another mode of observation is even more instructive. A *small* loose tangle of zoophytes, or of fine seaweed, or a very loose ball of tow, thrown into the water, is seen to rise and fall and to move to and fro; but though each part of it describes a circle, the mass does not rotate, nor yet do its parts move all together. One part of the mass is at the top of its circular orbit before another, and hence the mass changes in form. The changes of form, or deformations, may be most easily described if we suppose the mass to be such that in still water it would be spherical, that is, a sphere of water rendered visible by the suspension within it of loose tow or seaweed, or, better still,

mud-particles. When the water is disturbed by the passage of waves, the sphere becomes an ellipsoid, but it *does not mix* with the surrounding water. The longest axis of the ellipsoid is vertical when the mass is under (or within) the crest of a wave, and its shortest axis is horizontal and parallel to the direction in which the waves are travelling —this direction we will call longitudinal. The transverse axis of the ellipsoid, that is, the axis parallel to the crest of the wave, remains unchanged throughout, and therefore equal to the diameter of the spherical mass we started with. As this remains unaltered, we may leave it out from further consideration.

As the mass we are considering falls into the trough behind, that is, as the wave recedes from it, it comes to be elongated in a series of new directions. At first sight, the mass appears to rotate in the direction in which each of its parts revolves, so that the upper end of the long axis comes to be inclined forwards till when the bottom of the trough is reached this long axis is horizontal and longitudinal. The mass, however, does not really rotate : the point which was uppermost when the mass was at the crest of the wave is uppermost still when in the trough. The *form* of the ellipsoid rotates, while its substance does not. The long axis is vertical every time the mass is at the crest of a wave. In passing from one wave-crest to the next the *form* (not the substance) rotates through *half a revolution*. During the same period each particle performs a *complete circular* journey. The relative movement of any two particles in the mass is one of oscillation. Take, for instance, the uppermost and the lowermost particles of the spherical mass with which we started. When the water is still, one is vertically above the other. During the movement the upper one (A) moves in a circle, and the lower one (B) moves in a smaller circle. A is therefore above B when both are under the crest, and again when both are under the bottom of the trough. At every moment A and B are moving in the same direction, but A always faster than B. At the crest both are moving forwards, A therefore comes to be slightly in front of B, but as both turn to move backwards, A comes to be above B again in the trough and then comes to be behind it. If we take two other particles a similar result is reached, but with a difference. If the two particles are not in the same vertical transverse plane when at rest, then they will not at any moment be moving in the same direction. Suppose C and D be particles at the two ends of the "longitudinal" diameter of the sphere in still water, C being in front, D behind. Then as the waves advance D will always move somewhat before C, so that when the middle of the mass is under the crest D will already have begun to move downwards, while C is still moving upwards. At this stage C and D will also be nearer together than at any other. In the trough these will be further apart than at any other stage, and while both are moving backwards D will already be moving backwards and upwards, while C is still moving backwards and downwards.

The diameter of the circular orbit of a particle at the surface is equal to the height of the crests above the troughs, as has already been seen. The point which is of most importance to the jelly-fish, however, is the amount of movement and of deformation at various depths below the surface. This has been investigated by mathematicians, who express the result as follows :—Calling the diameter of the circular path of any particle the "extent" of its movement, and writing a for the extent of movement of a superficial particle, that is, for the height of the crests above the troughs, and λ for the distance between one crest and the next, then a particle which in the undisturbed water is at a distance h below the surface will in the disturbed water move to the extent expressed in the formula $ae^{-\frac{2\pi h}{\lambda}}$

At a quarter of a wave-length from the surface $4h = \lambda$, and the extent becomes $ae^{-\frac{\pi}{2}} = \frac{a}{5}$ or one-fifth of extent of movement at the surface. At half a wave-length below the surface $2h = \lambda$, and the extent, therefore, $ae^{-\pi}$ = one twenty-third of a. At one wave-length the extent is $ae^{-2\pi} = \frac{1}{535}$ of a. (The calculations are only rough approximations.)

From this it is obvious that, however violent the movement may be at the surface, there is always a zone to be found not far from the surface where the jelly-fish may swim and feed in safety, but above which it would be exposed to danger from the violence of the movements of the water. This zone we will call the "zone of safety," but it appears probable that the name "zone of maximum food-supply" would be equally appropriate. Its depth below the surface varies with the violence of the movements at the surface. In still water it is at the surface.

I have now to show reason for the belief that the tentaculocysts, without giving rise to any sensation, can automatically steer the jelly-fish in such way as to keep it close to this zone and to bring it back to this zone whenever it is forcibly carried away from it (as by currents), and that this will happen, whatever the distance of this zone below the surface may be.

Before considering how the tentaculocysts can serve this important function, I would once more remind my readers that the only waves we have been considering are waves—not breakers or "white horses"—in the deep ocean, and not in shallow water.

Suppose the jelly-fish to be some distance below this zone of safety and in water in which there is no appreciable disturbance, it will then, by virtue of the greater specific gravity of its oral portion, float with the mouth downward. Its rhythmic contraction will, under these circumstances, by increasing the mutual pressure between the water in the sub-umbrellar space and the inner surface of the umbrella, cause the animal to move upwards till it reaches a zone

where the movement of the water is great enough to tilt the animal into an oblique position.

In what zone this will happen depends upon conditions too complex for consideration here, but the direction in which the tilting will occur is a matter worth considering, and one which presents no great difficulty.

Each layer of water becomes thinner when under a trough of the surface-wave, and thicker when under a crest. The ratio of the velocities of two layers is constant so long as the surface disturbance remains unchanged. Hence it is obvious that as the jelly-fish extends through more layers when under a trough (where the layers are thinnest) than when under a crest, the difference in the rates of movement of the uppermost and lowermost of the layers occupied by the jelly-fish is greatest when under the trough, that is, when the movement is opposite in direction to that of the waves. Hence the alternate tilts in opposite directions will not be equal, and the total effect will be that the animal is turned with its aboral (or "upper") surface "up-storm," that is, towards the direction from which the waves are coming.

The rhythmic contractions continuing, the animal will swim upwards and in a direction opposite to that of the waves. As it does so it comes into new zones, each moving more rapidly than the previous ones, till at length a zone is reached where the changes in velocity are so great that the slight elasticity of the stalk of the tentaculocyst is no longer sufficient to keep the weighted tip from lagging so far behind the pouch as to touch its walls. At the moment when it touches, a stimulus is produced, which causes a more energetic contraction of the bell, especially in the adjacent portion of the bell. The tentaculocysts first so affected will, of course, be those exposed to most rapid changes of velocity, that is, those in the edge which is turned upwards. The vigorous contraction of this portion of the bell in advance of the contraction of the rest will turn the animal, with its aboral face, first, directly towards the point of the compass from which the waves proceed, and then gradually downwards, the swimming movements taking the animal to a greater depth, and in the direction opposite to that in which the waves travel.

When a deeper zone is reached, where the movement is too slight to bring the lithite, or weighted end of the tentacular portion of the tentaculocyst, into contact with the sides of the pouch, the greater specific gravity of the oral portion of the animal again turns the aboral surface somewhat upwards, and the animal again approaches the surface.

The animal is thus automatically steered so as to be kept always in the uppermost zone of safety—a zone of maximum food-supply; and this explanation involves no assumption as to consciousness, or sensation, or judgment on the part of the jelly-fish.

A pelagic mollusc, such as *Pterotrachea*, has a pair of "otocysts"

in close relation with a large nerve ganglion, or rather pair of nerve ganglia, with which the eyes and tentacles are also connected. The organisation of the mollusc is, moreover, incomparably higher than that of the jelly-fish, and it is not very rash to suppose that in this case the shaking of the animal by the movement of the water, and the consequent collision of the walls of the "otocyst" with the lithite ("otolith") gives rise to a sensation. Whether it does so or not is, however, of little moment. The ganglia are connected (indirectly) with muscles, and the stimulus produced by the collision of lithite and otocyst-wall may safely be assumed to lead to muscular movements, either reflex or voluntary—movement which will take the animal to safer zones of water.

A shaking of the "auditory sac" of a crustacean would, by jolting the "auditory" hairs against the lithites—mere sand particles—produce a stimulus. The shaking might be produced in either of two ways: it might arise from a tremor of the ground upon which, or in which, the animal is resting, or from a disturbance of the water about the flagella of the antennule. Of the advantage of being thus warned of the approach of an animal which either may serve as food for the crustacean, or might feed upon it if it were taken unawares, there need be no doubt.

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C. Herbert Hurst.

A CORRECTION:—

In the previous article in this series (p. 352, 19 lines from the bottom), for "500 miles" *read* "500,000 miles."

III.

Naegeli's Experiments on Living Cells.

A SHORT notice of Carl von Nägeli's paper, "Über oligodynamische Erscheinungen in lebenden zellen," concerning the fatal effect of "nominally" pure water on living cells, appeared in NATURAL SCIENCE (May, p. 333). The following is an account of the experiments by which Nägeli arrived at his conclusions. The record of this work, which was carried on in his laboratory at Munich, was found by Schwendener among his papers after his death. Professor Cramer, of Zürich, went carefully over the ground a second time and obtained the same results as his old master.

The research, we are told by Schwendener, dates from the year 1880. Dr. O. Löw and Dr. Bokorny had recently published a paper on the reduction of silver salts by living protoplasm. While verifying for himself the results arrived at by these gentlemen, Nägeli was led to turn his attention from the effect of living cells upon silver salts to the reverse action of the silver salts on living cells.

The *Spirogyra* experimented on is a very common fresh-water Alga; it is composed of long cylindrical filaments, divided up at regular short intervals into cells, with spiral parietal bands of chlorophyll. The spirals may be single or double, and they, as well as the cells, vary in size in the different species. These differences affected, to a considerable extent, the sensitiveness of the plant under examination; the more compact the spirals the greater the resisting power of the cells. It was also found that resistance to any hurtful influence was at its maximum towards the close of the day, when there was a great accumulation of assimilation products in the chlorophyll bands. In the early morning, when these were withdrawn and dispersed through the cells, the plant was much more sensitive.

The alkaline solution of the silver salt used by Nägeli in his experiment was the one prepared by Dr. Löw (containing 1 Ag NO_3, 1 NH_3, 3·6 K_2O to 100,000 volumes of water), which killed the plant by poisoning it; the cells lost their turgidity, the protoplasmic layer shrunk away from the walls, the chlorophyll bands changed colour and gradually became disintegrated. With further dilution the cells still died, but in a totally different manner; the turgidity of the cells and the protoplasmic layer remained unaffected, the chlorophyll bands alone seemed to be sensitive, they shrunk together with the

nucleus into a round ball. To this latter result Nägeli gave at first the name *Isagität*, but later he changed it to *Oligodynamic*.

As to the effect of solutions generally on *Spirogyra* and other cells, Nägeli enumerates three ways in which they may prove fatal—physically, by inducing plasmolysis, for instance; chemically, by poisoning the plant; and lastly, oligodynamically. The last-mentioned resulted when the chemical solution was so diluted that the reaction could not be one of simple poisoning, but must be due to some other influence.

Nägeli pursued the research still on the same lines; he put the filaments into Dr. Löw's solution, diluted quadrillion-fold, till, finally, he concluded that no calculable trace of the poisonous silver nitrate was present; the result was equally satisfactory from an oligodynamic point of view, the cells died often in less than four minutes. He now turned his attention to the water used in the solution, which was distilled in the ordinary way, and the conclusion arrived at was a startling one, namely, that pure water is a dangerous and hurtful fluid. After boiling, the evil effects diminished, they did not quite disappear.

Another series of experiments was tried with mercuric chloride—a still stronger poison. This was diluted down to septillion-fold, and the plants died as before. Professor Cramer has calculated that in order to dilute 1 milligramme of mercuric chloride to this extent, we should require more water than there is on our planet. A globe of water would be necessary thirteen million geographical miles in diameter, which would reach almost from the sun to Venus, and the molecules in such a solution would be separated from each other by a distance equal to two-thirds of the earth's diameter. He concluded, as we do, that the solution would be harmless, so far as mercuric chloride was concerned. This experiment proved conclusively that the evil was due to something other than chemical poison.

Attention was next turned to the glass vessels used, to see if the effect was produced by mechanical action. The glasses were tested in various ways, by movement, by covering them, etc., but no difference was visible in the behaviour of the solution; the *Spirogyra* died, and yet not always; in four out of this series of experiments the *Spirogyra* remained uninjured in the diluted solution. Again, boiling was tried, and in most cases the water was restored—we can hardly say to purity, but to harmlessness, or, as Nägeli terms it, it was rendered neutral.

Meanwhile, control experiments with pure distilled water had been carried on quite successfully in glasses full of *Spirogyra*. A culture was now tried with the same water and only a few filaments; as Nägeli expected, they died, if anything, still more quickly than in the diluted chemical solution, and water from the tap acted frequently exactly like distilled water.

A further research was now entered on to test the water, and as distilled water was, on the whole, more fatal than tap-water, he

fancied the poison might be a gas absorbed in the water and passing over in distillation. Carbonic acid, ammonia, and ozone were ruled out of the question, as any water in which Algæ live necessarily contains these gases in greater or less quantity, and such water, from spring or river, pool or pond, was found to be harmless. Moreover, when these gases were added to the cultures no effect followed. There remained nitrous acid to be suspected, and the more so that Munich water was said to contain a good deal of this acid which would not be eliminated by distillation. A solution was made containing potassium nitrite and sulphuric acid, which would liberate free acid in the distilled water. The same results followed as before, namely, chemical poisoning in the stronger solution, and the oligodynamic effect when much diluted. Griess's test was applied to estimate the quantity of nitrous acid in the ordinary water and in distilled water, but only once was there any reaction, and that so slight that no possible harm could be done by the amount present.

Very remarkable results in rendering water oligodynamic were obtained by treating it with various bodies considered practically insoluble in ordinary water. These were copper, silver, lead, iron, tin, and quicksilver. A whole series of experiments was tried with well-cleaned coins. Thus glass vessels, containing 100 or 500 cubic centimetres of water, were arranged with one, two, four, and eight gold coins. In these glasses were placed equal quantities of *Spirogyra* filaments. The plants in the glasses with the largest number of coins died first, the others succumbed more slowly, while the same species of *Spirogyra* lived in a healthy condition for weeks in ordinary spring water.

Again, the effect was tried with neutral water in metallic vessels of silver and platinum; the *Spirogyra* filaments died as before, and if copper coins were dropped in a glass containing the filaments the effect was so far localised that those nearest the coins were attacked first.

Nägeli had thus found an easy method of rendering water oligodynamic. A step further, and he made the still more remarkable discovery that he could again render the solutions neutral by the addition of quite insoluble bodies, trying first sulphur, soot, or graphite, then manganese, starch, flour, cellulose (filter-paper, cotton, linen, or wood), silk, wool, stearin, paraffin, etc., and the more of these bodies he put in, the quicker did the water recover its neutrality. He found that Algæ themselves would act in the same way if he put in plenty of them; they, too, had power to render the water neutral. This enabled him to account for much that had been hitherto inexplicable in the control experiments, where no proportion had been observed between the relative quantities of Algæ and water. This was put definitely to the proof, and it was found that the more water used, and the fewer the filaments, so much the quicker did oligodynamic reaction take place. Colloid substances, such as gum,

dextrin, albumen, glue, were also successfully employed as neutralising agents; while sugar and salt, which dissolved in the water, were almost wholly ineffective.

Results were for a time much confused by the persistence of after-effects in the vessels. The glasses in which coins had been immersed retained, to some degree, oligodynamic power, even after careful cleansing, and the effect continued, all unknown to the experimenters, during three or four subsequent cultures.

Nägeli had now arrived at what seemed to be a deadlock; that almost insoluble substances should exert so fatal a power, and that quite insoluble substances should be effective in counteracting it, was very mysterious.

Would electricity, or some such imponderable agent, he asked, account for the facts? And so there were started new experiments to put this theory to the test.

The first bore upon Temperature; but here again he was baffled, as neither excessive heat nor sudden change accounted for the hidden force.

The effect of Light was questioned, and that also gave no response.

There remained Electricity. Had it been generated in the water by the metals employed, and did *Spirogyra* cells act as an electroscope, exceeding anything hitherto known in sensitiveness? This theory also was found untenable. Vessels of water were separately charged with positive and negative electricity, and the plants were put in. They remained unaffected, and equally so when a strong induction current of electricity was passed through a tube in which filaments had been placed. The plants were further exposed to the direct action of electricity from a battery; this caused the cells to swell, and induced other changes, but it in nowise resembled the action of oligodynamics.

The only conclusion left was that here was a new power not to be accounted for by any present theories; either a new agent was at work, or the old agents had developed new properties.

The problem was attacked again, to see if the metals above-mentioned acted in solution or in mass. Gold and platinum are quite insoluble; they had already been tested, but the gold coins used were alloyed with copper. Pure gold was prepared from gold chloride, and the platinum was treated with hydrochloric acid; they were then found to be harmless, and also it was now proved that washing with acid would neutralise oligodynamic glasses. Thus it was, in all probability, some barely soluble substance, such as copper, that had to be dealt with.

Oligodynamic water was analysed, and was found to contain lead, zinc, copper, and iron. Some copper coins were left for a stated time in water till it became strongly oligodynamic. On analysis, the solution yielded one volume of copper to seventy-seven million volumes of water.

Twelve pennies were left four days in twelve litres of water, which then had a slightly metallic taste, and the *Spirogyra* plant died in it in one minute. The water generally used in experiment had been only one-fifth or one-tenth so strongly oligodynamic. Indeed, one part of copper in one thousand million parts of water may be fatal to *Spirogyra* cells. The copper went into solution as cupric hydrate combined with carbonic acid. Other metals, silver, zinc, iron, lead, mercury, acted similarly. The salts of these metals could also render water oligodynamic, and the vessels that had contained the solutions retained oligodynamic power, though to a less marked extent.

According to physicists, the saturation of a solution depends on the quantity of substance that a definite quantity of water will take up; as the solution becomes supersaturated, equilibrium is restored by the redeposition of the substance. The molecules of a barely soluble body tend to attach themselves to some other substance, and so pass out of solution. Thus copper, in water which contains carbonic or other acid, gives off its molecules slowly but continuously. These spread through the water, some attaching themselves to the wall of the vessel. As complete saturation is reached, still more molecules adhere to the walls. Should the copper be removed before this stage is reached, the water withdraws some of the molecules from the wall and an equilibrium is established between the copper layer and the solution. If such a solution be poured into a clean glass, a layer of copper molecules is again deposited, and the greater area of wall offered, the more molecules will be attracted from the solution.

In this lies the explanation of the neutralising effect of the various bodies above-mentioned. They simply attracted the metallic molecules. When oligodynamic water was well shaken up with powdered sulphur, and filtered, it was rendered completely neutral, and thus also sugar or salt failed, as they themselves pass readily into solution.

As to the action of gum, albumen, etc., Nägeli explains it on his own "micellar" theory of the structure of organised bodies, of the truth of which he finds here additional evidence. He considers that organised bodies are built up of micellæ, invisible crystalline bodies, formed by the aggregation of chemical molecules, and that colloid substances form with water "micellar solutions"; the copper molecules attached themselves to the micellæ as they would to larger bodies. The Algæ themselves acted as neutralising agents when their number was in excess, and in this case the molecules could only act very slowly or not at all.

Metallic substances would, in the same way, be removed from lakes, rivers, etc., by the presence of large quantities of insoluble substances.

The water used in the experiment was supplied by the town (Munich). It was conducted through lead pipes terminating in the

laboratory in a brass tap. The first litres drawn were strongly oligodynamic, from contact with the lead and with the copper of the tap; after some quantity had been run off, the water was neutral. The distilled water was rendered hurtful by the apparatus used. Neutral water could be obtained at any time by distilling in glass alone.

Nägeli's papers on oligodynamics were handed to Professor Cramer of Zürich in the spring of 1892, who at once instituted a careful examination of the recorded experiments, with the result that this peculiar power of weak metallic solutions was completely verified. He followed very much on the lines of the previous research, testing the effect on *Spirogyra* cells of copper, copper sulphate, mercury, mercuric chloride, and Dr. Löw's solution of alkaline silver nitrate, and the results, with slight variations, corresponded to those already determined by Nägeli.

The water used in Zürich comes from the lake, is exceptionally pure, and is conveyed to the town in iron pipes. With the exception of the first litre drawn, which had been in contact with the brass tap of the laboratory, it was, in Nägeli's sense, absolutely neutral. The distilled water usually employed was prepared in a copper vessel lined with tin. It reacted variously, and especially seemed to lose oligodynamic power if it had been in several vessels, or if it had stood some time after preparation. In this connection, we may also note that he found the oligodynamic reaction was in proportion to the size of the vessel used in experiments. If there was much of the metallic solution in a small vessel, the effect on the cells was rapid; the same solution acted much more slowly when a drop of it was used on a glass slide where the glass surface exposed was so much greater in proportion to the quantity of metallic molecules present. The solution became weaker the longer it was allowed to stand, and repeated boiling or filtering neutralised it altogether. Water distilled in glass was, he also found, invariably neutral.

Professor Cramer chose, for the purpose of research, three species of *Spirogyra*; one of these, *Sp. quinina*, is of a delicate texture with only one spiral chlorophyll band, the other two, *Sp. densa* and *Sp. setiformis*, are coarser. The reaction depended, to a certain extent, on the species; *Sp. setiformis*, with closely-wound spirals, always resisted the action of the metal longer than the other two species. The sensitiveness of the plant was, however, intensified if it had been long under cultivation in the warm temperature of the laboratory, in which case the cells lengthened, and the spirals were, in consequence, looser and more sparse. He used for his control cultures large glass vessels filled with ordinary Zürich water.

Oligodynamic reaction was most easily obtained by water in which copper had remained from one to three days. If a healthy plant growing in neutral water were strewn with copper turnings, the chlorophyll bands very quickly shrunk from the sides of the cells in contact with the copper. A glass slide that had been in contact

with the moist copper stage of the microscope proved strongly oligodynamic after an interval of sixteen days.

Professor Cramer directed his attention chiefly to the action of copper and mercury upon water; he obtained them chemically pure, and placed them in vessels, some of which were carefully stoppered and full to the brim with neutral distilled water, others were only half full, and were supplied with oxygen and carbonic acid. The water in both cases became oligodynamic, the latter more powerfully; but though, in the former, the air had been carefully excluded, the water gave an acid reaction. Zürich water, which was chemically neutral, contained very little chalk, but there was enough to cause discolouration, as the carbonic acid, which had held the lime in solution, was set free and passed over with distillation. Besides this acid, Professor Cramer thinks there might be traces of silicic acid present.

In addition to the neutralising agents enumerated by Nägeli, it was found that iron rust was very effective, as also *Leptothrix ochracea*, an Alga belonging to the Schizophytæ rich in ferric hydrate.

With the various solutions the same effects were produced as already recorded; first that of chemical poisoning in the cells when much of the deleterious agent was present, followed by the equally fatal oligodynamic reaction in diluted solutions. At a certain stage of dilution, if neutral water were employed, all reaction ceased.

Annie Lorrain Smith.

IV.

Some Extinct Sharks and Ganoid Fishes.[1]

OUR conceptions of some of the more primitive groups of fishes have been greatly modified during the past few years. The discovery of nearly complete skeletons has shown that the fragmentary fossils originally compared with the corresponding hard parts of existing animals truly belong to strange races of which no idea could be formed from their examination alone. Sharks, sturgeons, mud-fishes, and the fringe-finned ganoids, in Palæozoic times seem to have flourished under almost as many modifications as characterise the modern bony fishes in the fauna of to-day; and it becomes more and more evident, as discoveries continue, that the common ancestors of the class date back to a period hopelessly remote, so far as the palæontologist is concerned.

Among those who have contributed an important share to the formation of these new conceptions, Dr. Anton Fritsch, of Prague, merits our special gratitude. He has for many years collected the fossil vertebrata from the Permian gas-coal formation of Bohemia, even preserving the pyritised specimens by removing the decayed bone and taking electrotypes of the impressions; and his great work descriptive of these fossils is the most painstaking and elaborately illustrated monograph of its kind. Quite lately, a new instalment of the third volume has appeared, and the account of the fishes is now extended to include the Acanthodians and the first section of the Palæoniscids.

Several years ago, in an earlier section of his work, Dr. Fritsch published the first detailed description of the singular primitive sharks known as the Ichthyotomi, supplementing the beautiful figures and brief notes of M. Charles Brongniart in his monograph on similar fossils from the French Coal-measures. Now, he adds further to our knowledge of the sharks by a detailed chapter on the Acanthodians; though in this case, unfortunately, he is able to contribute comparatively few new facts of fundamental importance, merely confirming, revising, and systematising the observations of previous authors.

[1] Fauna der Gaskohle und der Kalksteine der Perm-formation Bohmens. Vol. iii., pt. 2. By Professor Anton Fritsch Prague: F. Rivnac, 1893.

The Acanthodian fishes, discovered in Palæozoic rocks from the Lower Devonian upwards, have always presented difficulties to systematists; but it is now generally agreed that they are a peculiar order of sharks. As to the precise significance of several parts of their skeleton, however, there is still much disagreement, and Dr. Fritsch himself wisely refrains from very definite expressions of opinion.

The general aspect of the type-genus *Acanthodes* is shown in Dr. Fritsch's outline figure reproduced below. The cartilages of the head are calcified with granulations, as in sharks; and there are splints covering the jaws, but neither these nor any other skeletal parts have the structure of bone. The roof of the skull is covered at least in part with small dermal plates, and there is a ring of plates round the eye. The gill-arches are so much crowded that Dr. Fritsch thinks the gill-slits must have been covered by a flap of skin, as in the existing Chimæroids. The notochord must have been persistent, and there are rarely traces of the neural and hæmal arches above and below the vacant space it once occupied. The tail is heterocercal,

Fig. 1.—Outline restoration of *Acanthodes bronni*; Permian, Rhenish Prussia. (*After* Fritsch.)

the upper lobe being much produced; and in front of all the fins, except the caudal, there is a strong spine. The body is covered with little quadrangular scales, consisting of dentine enamelled at the surface; and the "lateral line" does not pierce these scales, but runs between two series.

Among the Permian Acanthodians Dr. Fritsch recognises three genera, one of which is destitute of pelvic fins. The large size of the paired fins, however, is one of the chief characteristics of *Acanthodes* itself; and, as pointed out by Professor Cope, there is more evidence of a continuous pair of lateral fin-folds in these Acanthodians than in any other fishes. Some of the Devonian genera (*e.g.*, *Climatius*), exhibit between the pectoral and pelvic fins a close and regular series of paired spines, in every respect identical with those supporting the fins themselves; and it is quite possible that these intermediate spines may also have been connected with fin-membrane.

It is strange to find a group of fishes like the Acanthodians among the sharks—fishes which at the present day never exhibit structures of the nature of membrane-bones, and never have the

cartilages of their limbs excessively abbreviated. It is, however, stranger still to have to regard the Palæoniscidæ as primitive representatives of the modern sturgeons; and yet Dr. Fritsch, following all who have deeply studied the subject, is fully convinced of the necessity of this course. He describes the genus *Trissolepis*, giving the restoration copied below; and this he regards as the type of a distinct family to be placed in the same sub-order as *Palæoniscus* itself.

The Palæoniscidæ, as a general rule, conform to the type shown in Fig. 2; being essentially sturgeons (as Dr. Traquair first pointed out) covered with regular rhombic scales and provided with a complete

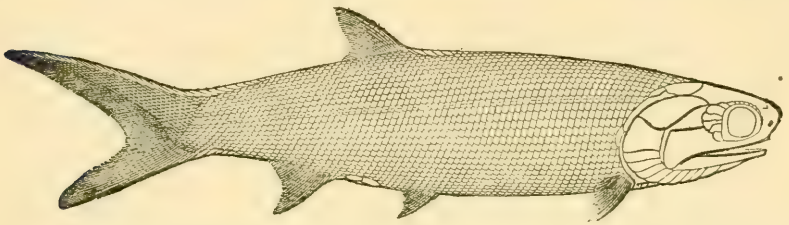

FIG. 2.—Restoration of *Palæoniscus macropomus*; Permian, Germany. (*After* Traquair.)

gill-covering apparatus. In some cases, however, the scales disappear except on the upper lobe of the tail; and in a single instance, briefly noticed by Dr. Traquair, the scales become deeply overlapping and almost or quite cycloidal.

Now, the great interest of Dr. Fritsch's new contribution to the subject consists in the fact that it is the first detailed and illustrated account of one of these ancestral sturgeons possessing cycloidal scales. Moreover, it is to be noted that in *Trissolepis* the cycloidal scales are confined to the trunk, while the rhombic scales persist on the upper lobe of the tail. One and the same fish thus displays

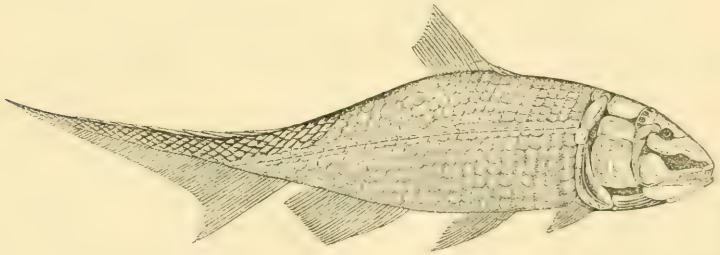

FIG. 3.—Restoration of *Trissolepis kounoviensis*; Permian, Bohemia. (*After* Fritsch.)

features which were not long ago regarded as the essential characters of distinct groups; and the shape of the scales in future must only be cited among the minor diagnostic points of the old "ganoid" fishes. We are acquainted with Jurassic fishes, indeed, in which the anterior scales are rhombic, ribbed, and united by peg-and-socket joints,

while the scales on the tail of the same fish are as thin and deeply overlapping as in a herring.

In a general review it does not seem appropriate to enter more closely into the details of Dr. Fritsch's work; Dr. Traquair has already published a critical notice in the *Geological Magazine* (dec. iii., vol. x., pp. 175–178). We would, however, urge all who are interested in Comparative Anatomy to study the original for themselves.

A. SMITH WOODWARD.

V.

Notes on Warning and Protective Colour in Lepidopterous Larvæ.

IN the April number of NATURAL SCIENCE, Mr. George Carpenter deals with the vexed question of "Colour in Insects." The whole subject of animal colouration has been much debated, and as it is only by means of a long series of careful experiments and close observation that we can hope to arrive at any clear understanding of it, every particle of evidence, however small, may have some value. From this point of view the following notes may not be without interest.

The experiments made by me as to palatability and colour-relation in lepidopterous larvæ were, as Mr. Carpenter correctly states, entirely confirmatory of those previously conducted by Mr. Poulton and others. They have been recorded in detail elsewhere (*Trans. Ent. Soc. Lond.*, 1892, pt. iii.), and I therefore give only a brief sketch of them here, together with a few additional facts which have come under my notice since writing the above-mentioned paper. Experiments as to the palatability of conspicuous larvæ were made with four species, viz., with larvæ of *Diloba caeruleocephala*, *Cucullia verbasci*, *Acronycta psi*, and *Bombyx rubi*. Larvæ of the two first-named species were given to a tame jackdaw, which had been taken unfledged the previous year, and could never have seen any larvæ except those I gave him, unless some had dropped occasionally from a beech tree which overhung his cage. The larvæ of *D. caeruleocephala* were taken feeding freely exposed on pear trees; they were blue, yellow, and black, very conspicuous, and not hairy.

The bird was accustomed to take anything from my hand, and would always seize and attempt to pull to pieces any object presented to him, whether he was hungry or not; he was, therefore, a good subject for experiment.

On offering him a larva he appeared very suspicious, and for a long time would not take it. Then he tasted it, but dropped it at once, shaking his head violently, and evidently disliking it. It was tasted once more, and then left uneaten. The next day he was offered a common smooth green larva (species unknown) and ate it without hesitation. He was not very hungry on either occasion.

Larvæ of *C. verbasci* were taken feeding exposed on the upper side of leaves of mullein; they were green, yellow, and black, very conspicuous, and not hairy. One was offered to the jackdaw when the bird was very hungry; it was refused at first, then tasted once only, and dropped with greater signs of disgust than in the case of *D. caeruleocephala*. After this he would not take anything at all from me for a considerable time, and appeared very uncomfortable.

In the case of *Acronycta psi* and *Bombyx rubi* the animals experimented on were three slowworms (*Anguis fragilis*) and one lizard (*Zootoca vivipara*). These were purposely kept very hungry, but though the larvæ were left with them for three days they refused them entirely, never even attempting to taste them. Both these larvæ, however, were hairy as well as conspicuous, and, therefore—assuming that conspicuousness means unpalatability—were doubly protected. The fact that an animal may possess more than one kind of protection must of course be taken into account in making these experiments, and for this reason the experiments with *D. caeruleocephala* and *C. verbasci* were the most satisfactory, because here the unpleasant attitude was almost certainly taste only. Mr. Poulton says (*Proc. Zool. Soc. Lond.*, March 1, 1887) that Mr. Jenner Weir found the two last-named larvæ to be disregarded both by birds and lizards. This certainly supports Professor Wallace's original suggestion of the unpalatability of conspicuous larvæ to "some, at least, of their enemies," while, on the other hand, the results obtained by me also favoured Mr. Poulton's suggestion as to hunger putting a limit to this method of defence, since the larvæ were tasted, and tasting would be as fatal to them as eating.

Mr. Beddard ("Animal Colouration," p. 164) found that *A. psi* was eaten by *Lacerta viridis*, and at least tasted by a thrush, but he does not mention whether either was hungry on these occasions, a point of some importance. In any case, his results differed from mine in this experiment, but then a different species of lizard was used. *L. viridis* would seem, from other experiments of Mr. Beddard's, to be less sensitive to unpleasant attributes than other lizards, *Z. vivipara*, for instance. This difference of behaviour does, as Mr. Beddard points out (*loc. cit.*, p. 155), show that unpalatable animals with warning colours are not always exempt from attack, but this very fact rather tends to support the view held by Mr. Poulton that this means of defence can only safely be adopted by a limited number. On the other hand, it militates strongly against Dr. Eirig's theory, quoted by Mr. Beddard, that brilliant colours (*i.e.*, abundant secretion of pigment) are the cause of inedibility. Exceptional cases like that of *L. viridis* eating *Acronycta psi* are difficult to explain on this hypothesis, but they appear more comprehensible on the theory of limitation. *A. psi* might be regarded as one of the few examples of the failure as a means of defence both of the unpleasant attribute and its advertising warning colours, and *L. viridis* as having had its power of

eating this larva with impunity developed by the action of Natural Selection, after necessity had first driven its ancestors to the attempt.

My experiments were made on a less extensive scale than those described by Mr. Beddard as undertaken by Mr. Finn and himself, and are, therefore, perhaps, of less value; but as far as my experience went I was not at all able to confirm Mr. Beddard's statements as to the "behaviour of animals when offered inconspicuously-coloured" prey, resembling that with which they approached conspicuous forms. I repeatedly gave common green smooth larvæ and other species with protective colouration both to the bird and lizards, and invariably found them accepted and eaten without the slightest hesitation, except just after distasteful food. I scarcely think the garden slug, which Mr. Beddard saw refused by several birds, is a fair example of a protectively-coloured animal treated as if it were inedible. The slime of the slug would be disliked by many animals, and there is evidence from many of the experiments that it was so disliked, since it was only swallowed "after much rubbing on the ground." The slime would seem to be as effectual a protection, apart from all question of colour, against some pursuers as the hairs of the larva of *Bombyx rubi*; the latter cannot be said to be conspicuously-coloured, and yet was refused by all the animals with which I experimented. I did not find a single case of a larva which was both protectively-coloured *and smooth* being refused by animals; such larvæ were always eaten with avidity. It will, however, be interesting to extend these experiments and note the results carefully, and it is one of the great merits of Mr. Beddard's book that he therein points out so many lines for investigation, and calls attention to many details which are worthy of further consideration.

With regard to experiments on the colour-relation between certain lepidopterous larvæ and their surroundings, the results obtained by me were the same as those obtained by Mr. Poulton previously with the same species, except that in one case my results were, perhaps, slightly more definite.

I reared in different surroundings 19 larvæ of *Rumia cratoegata*, 42 larvæ of *Catocala nupta*, 10 larvæ of *Catocala fraxini*, and 29 of *Mamestra brassicæ*. Dark surroundings produced in *R. cratoegata* very dark brown or nearly black larvæ, while in green surroundings larvæ from the same parent became of varying shades of green or greenish-brown, with touches of crimson exactly corresponding to the red of the thorns and one side of the young shoots of the food-plant. The resemblance to twigs of hawthorn was very perfect, and was heightened by the angular attitude adopted by the larvæ at rest.

For *Catocala nupta*, dark, green, and white surroundings were used, and the dark surroundings produced darker brown larvæ than the green surroundings, while no difference in shade was perceptible between those in green and those in white surroundings. Mr. Poulton

cons'ders (*Trans. Ent. Soc. Lond.*, 1892, pt. iv.) this species to be less susceptible than other Catocalidæ.

Catocala fraxini certainly showed a distinct difference, not only of shade, but of colour, larvæ reared in green surroundings being distinctly greenish in hue, while those in dark were brownish. Mr. Poulton obtained only a difference in shade, otherwise his results were similar.

With *Mamestra brassicæ* I had negative results ; the larvæ were not affected at all by surroundings, and I believe this unsusceptibility to be due to the burying habits of this species, which render colour of secondary importance to the larvæ. Mr. Poulton's results differed a little from mine, but he thought that his larvæ were not in such a healthy condition as mine, which might account for this. There are, however, special difficulties in investigating *M. brassicæ*, and Mr. Poulton considers the experiment worth repeating.

Since publishing these results, I made some experiments, at Mr. Poulton's suggestion, on larvæ of *Triphena pronuba*, which I was rearing for him for other purposes. I had 59 larvæ in different surroundings, and as this species, like *M. brassicæ*, buries by day, I used different-coloured materials on the floor of the cylinder, such as white sand, yellow gravel, pounded red flower-pot, and, in one case, small coal. The larvæ buried in all these, except in the coal, which they evidently disliked, but the results were, as in *M. brassicæ*, negative, with two possible exceptions from green surroundings, without any burying materials ; these were, however, not very definite. The burying materials were in all cases used in addition to sticks of the suitable colour mixed with the food-plant.

Besides the foregoing experiments, I also made some notes on the red spots in *Smerinthus* larvæ, from larvæ of *S. tiliæ* sent me by Mr. Perkins at Mr. Poulton's request. These larvæ were all from parents which had been spotted in the larval stage. Unfortunately, many were injured by their transit by post, and I only succeeded in rearing four, which were all spotted. I also captured a larva of *S. populi* in which the spots were extremely well marked. The spots were certainly protective in effect, on account of their resemblance to the dark spots or blotches common on leaves of poplar, and this fact was long ago pointed out by Mr. Peter Cameron (*Trans. Ent. Soc. Lond.*, 1880) ; but although he considered the likeness to *galls* (*Phytoptus*) very striking, it seems to me, in *S. populi* at any rate, far greater to flat spots on the leaf seen with the light shining through them.

In *S. tiliæ* I studied the development of the spots very carefully, and in one individual I found them very linear and strongly suggestive of coloured borders ; in the others they were much bolder and rounder, and presented quite a different appearance. Professor Weismann, as is well-known, considers the spots as on the way to develop into borders, while Mr. Poulton takes the view that we

have to do with a gradual contraction of borders into spot-marking. The spots certainly increase in area while developing, and if we are to regard the occurrence of spots as a recapitulatory character this fact would seem to support Professor Weismann's theory. If, however, they are reversionary, which now seems to me more probable, as they do not occur in all the individuals of a species, the increase in area would not have the same importance. The number of larvæ reared by me was, however, too limited to draw conclusions from safely, and had I had more individuals under observation, some might have presented different appearances.

Lilian J. Gould.

VI.

Cannibalism among Insects.[1]

IT is known that the caterpillars and larvæ of lepidoptera, crickets, and locusts feed on plants, of which they consume great quantities, in order to reach their full development or to prolong the imago condition, and to fulfil their mission of propagating the species.

Notwithstanding the vegetarianism of the insects mentioned, cases are known in which they abandon their usual habits and become addicted to a flesh diet, and, what is still more strange, they feed upon their own kind.

The larvicide instincts of the caterpillars of certain moths have been known for a long time, the cannibalism having been observed in various species deprived of their liberty, and brought up in confinement, either with or without vegetable food. Thus, for example, the caterpillars of the moths *Calymnia trapezina*,[2] *Agrotis ypsilon*, *Heliothis armiger*, and others, finding themselves in captivity, abandon their vegetable regimen and devour their companions, whether out of disgust at finding themselves imprisoned, or for want of fresh vegetable food, or for other causes of which we are ignorant. In any case, after the development of the disordered appetite, the caterpillars do not any longer care for vegetable aliment, and they endeavour to satisfy, in any way, their newly-acquired carnivorous habit. Out of a number of caterpillars associated together, of the above-named species, finally only one remains, which in the savage warfare, either by its strength or subtlety, has succeeded in saving itself, devouring the last of its companions which placed its existence in danger.

What has been just said only relates to caterpillars in captivity. So much greater is the fact which I discovered, that in the free condition there also exists cannibalistic larvicide. During my voyage in Southern Patagonia, in 1874, I noticed that the cannibalistic instinct was very prominent in the caterpillars of the district, whether free or imprisoned.

[1] Translated from the *Anales de la Sociedad Cientifica Argentina*, Nov., 1892, pp. 230–238.

[2] The caterpillars of this moth are said to be often carnivorous in the wild state, and to render considerable service to fruit-growers by devouring the destructive larvæ of *Cheimatobia brumata*.—Ed.

All the caterpillars belonging to a particular group or family showed a preference for the flesh of their own relations. They devoured one another in considerable numbers, rarely feeding on the plants suitable for their nourishment. The caterpillars of the family of the Bombyces devoured their kin with skin and hair, and they went as far as to break through the cocoons in order to finish with the chrysalis within.

In a similar manner, the caterpillars of the Noctuids behaved with those of their own kind and with those of the Bombyces.

The caterpillars of these latter attacked also those of the former without any exception. The most voracious of the Noctuids was the caterpillar of *Heliothis armiger*; a single one of these consumed, in twenty-four hours, six or seven other caterpillars.

The caterpillar of the well-known butterfly, *Pyrameis carye*, is also a carnivorous cannibal, but with moderation, preferring always fresh plants to meat, while the others, and chiefly those of the Noctuinæ, after having tasted the flesh of their own kin, would no longer touch vegetable food.

I explain this particular character of the Patagonian caterpillars in the following manner. During the principal part of the summer in Patagonia there is considerable heat and dryness which causes the vegetation quickly to be parched up and scarce. When this happens the caterpillars lose their means of subsistence, but, in order that some may survive, the struggle for existence has taught them another means of subsistence, that is, the flesh of their own kind. Once this instinct has been inherited, the descendants will use it whenever an occasion presents itself, and, in many instances, even when there is no lack of vegetable food. In some cases it is necessity which produces the habit; in others, inheritance which leads up to it; thus new biological characters are formed.

Of other herbivorous insects cannibalism has been noticed in crickets in confinement. The first notice of this strange taste we owe to Sir W. Brodie (*Canad. Entom.* vol. xxiii., p. 137, 1891), whose observation has been recently confirmed by Mr. Philip Laurent (*Entom. News*, vol. ii., p. 180, 1891).

In an assemblage of many crickets kept for certain observations in a rearing drawer, or box (caja de herborizacion), the numbers diminished from day to day; at last only one—not a little fattened—remained by the side of the remains of his former companions.

Hitherto cannibalism among the crickets has been noticed only among captives, but I am now enabled to state that, under certain conditions, cannibalism is present among some Orthoptera in the free state, at all events among the locusts.

In the summer of 1883, in which the excessive heat and drought had brought about the nearly entire disappearance of the vegetation in a good part of the country, and more particularly in the broken country of the Banda Oriental, I had occasion to make a journey

from San José to Mercedes. At one place, Las Piedras, at which the diligence stopped, I noticed great numbers of locusts of the species *Pezotettix vittiger*, *P. maculipennis*, and *P. arrogans*, which covered the ground and rocks. My attention was attracted by the fact of seeing around one locust a number of other individuals of the same species, which were eating its soft parts even while it was yet alive and protesting vigorously. I saw different attacks, in which the conquerors, two or three at a time, got hold of the weaker members of their own kind, throwing them over, and opening the abdomen in order to devour the entrails, these being the softer and more savoury portions, since they still contained some of the vegetable food. Cannibalism here appeared in its lowest development, and the numerous remains of those which had been eaten bore witness to the extent to which the process had been carried.

In the face of facts of this character, it seems certain that nothing is sacred in Nature, when the prolongation of life, for the sake of the preservation of the species, is concerned.

CARL BERG.

VII.

The Classification of Arachnids.

NATURALISTS have long looked forward to a day when the systematic arrangement of living creatures will exhibit the real relationship which they bear to one another. All honest classificatory work helps science to realise this great ideal, but much is of the nature of raw material which may long remain without apparent use. Occasionally, however, we meet with contributions which seem to add considerably, and at once, to the progress of the work; and such are several recent memoirs on the spiders and their allies.

In 1851, Schiödte described a new genus of Malayan spiders, *Liphistius*, of most remarkable structure. This genus was at first classed with the Territellariæ—the tribe which includes the trap-door species, and the well-known large hairy "bird-killers" of the tropics. More recently Thorell has considered that the genus should form a tribe by itself, but be included with the Territellariæ in the sub-order Tetrapneumones, or spiders with four lung-sacs; and now Pocock (1) gives good reasons for assigning to *Liphistius* a still greater classificatory value, and placing it in solitary grandeur over against all other known spiders. Several striking characters seem to support this view. The abdomen in other spiders has lost nearly all traces of segmentation, but *Liphistius* exhibits nine dorsal and two ventral sclerites. The spinning-mammillæ, which in other spiders leave their embryonic position as ventral appendages of the abdominal sternites behind the lung-sacs, and migrate to the extreme apex of the abdomen, retain in *Liphistius* their primitive place beneath the middle of the abdomen. This latter character suggests the names proposed by Pocock for his new divisions, the species of *Liphistius* being called Mesothelæ, and other spiders Opisthothelæ. His primary division of these latter tallies with the old classification into Tetrapneumones and Dipneumones, but he agrees with Simon that the relationships of a genus (*Hypochilus*) of four-lunged spiders are with the latter rather than with the former group, and, therefore, suggests the terms Mygalomorphæ and Arachnomorphæ to supersede those time-honoured names. In all spiders of the latter group (except *Hypochilus*) the hinder pair of lung-sacs are replaced by tracheæ. *Liphistius* agrees with the Mygalomorphæ in having four lung-sacs, but, in several points, it shows affinities with the other division. Alone

among spiders it possesses four pairs of spinning mammillæ. While the Mygalomorphæ rarely have more than two pairs, the Arachnomorphæ always have three, and Pocock thinks that the extra central spinning organ (cribellum), found in some families of the latter, represents the fourth pair of spinners of *Liphistius* fused together.

In this new classification, therefore, the archaic characters of *Liphistius* are emphasised, and it is regarded as a survival of an ancestral form of spider in which the abdominal segments have not yet become fused, and still bear the spinners on their ventral aspect. The animal is, to some extent, a connecting link between the spiders and the scorpion-spiders (Pedipalpi). We are thus led to consider the relationships which exist between the various orders of the Arachnida—a subject also dealt with by Pocock in a later paper (2). His principal contention is that if arachnids are descended from ancestors which must have possessed a long series of nearly similar segments, those modern representatives of the groups in which the segmentation is best preserved must be the most primitive. On these grounds he considers the scorpions as the lowest living branch of the arachnid stem, in opposition to the views of Thorell and others, who have regarded them as the highest. There can be no doubt that Pocock's opinion will meet with general acceptance among biologists.

Deriving the arachnid orders immediately from an ancestor with a long abdomen of twelve segments and a telson, Pocock indicates the modifications which have probably occurred in each group. In the scorpions these segments are all present, but the hinder six are much reduced in bulk to form the post-abdomen, the telson being utilised as the sting. In the scorpion-spiders there has been a great reduction of the hindermost segments; the tail in *Thelyphonus* represents the telson. *Liphistius*, with its much-shortened and partially segmented abdomen, connects these with the higher spiders in which the abdominal segments are completely fused together. Nearly related to the Pedipalpi, but with tracheæ instead of lung-sacs, are the Solifugæ[1] and false-scorpions (Chelifers), with the number of abdominal segments complete, or but slightly reduced. On a higher branch of the stem bearing these come the harvestmen (Phalangida), with the abdominal segments generally reduced in number, and fused with the cephalothorax anteriorly. From these the mites (Acari), with the whole body fused together, must probably be regarded as a degraded offshoot.

The relation of the mites to the rest of the Arachnida has been also recently discussed by Bernard (3), who gives reasons for believing that these creatures are not examples of true degradation, but that they have become fixed at a larval stage of development. Comparing their digestive, vascular, and nervous systems with those of the

[1] Bernard (*A. M. N. H.* (6), vol. xi., 1893, pp. 28–30) has announced recently the discovery of a sensory organ on the pedipalp of *Phrynus*, similar to that on the homologous appendage of *Galeodes*.

higher Arachnids, he shows that they want the greater number of the abdominal segments, while they possess, in a fair state of development, organs homologous with those in the anterior part of the body of the higher Arachnids. He speaks of the Mites as "larval Araneids," but few naturalists will be inclined to regard them as a direct offshoot from the spiders. Extreme smallness—one of the supposed advantages gained by fixation at the larval stage—has been acquired by many true spiders. Moreover, spiders, like most other Arachnids, have no larval stage, but leave the egg in a form similar to that of the adult animal. Whether degraded or arrested, the affinities of the mites seem to be with the harvestmen, from which group they are not readily defined.

Trouessart (4), however, supports the view that, on account of their development with a metamorphosis, the mites should form a special sub-class of the Arachnida, and he separates the vermiform mites, such as the Phytopti, from the rest as a separate order; but the larval stage of mites (in which they have but three pairs of limbs) seems quite a secondary adaptation, for, according to Wagner (5) the fourth pair appear in the embryo, to be afterwards concealed beneath the skin of the larva and then to re-appear during metamorphosis.

It is remarkable that the theory of fixation at the larval stage has also been invoked by Von Kennel (6) to account for the origin of the microscopic "water bears" (Tardigrada), which have generally been regarded as degraded arachnids. He compares them with certain midge-larvæ (*Cecidomyia*) which have the power of parthenogenetic reproduction, and suggests that if male organs were also precociously developed a species might become permanently larval in form. Though he does not assert that the Tardigrada are "arrested" flies, the acceptance of his views—which certainly have much to support them—would probably lead to the removal of the group from the arachnids to the insects, in which the larval stage is often of such supreme importance.

The replacement of lung-sacs by tracheæ in several groups of arachnids opens up a question of much interest. Reference has been made in NATURAL SCIENCE (vol i., p. 524) to the supposed origin of the lung-sacs from the invagination or enclosure of gill-bearing appendages. If this view be accepted, we must regard the abdominal tracheæ in the higher spiders, harvestmen, false-scorpions, etc., as simplified lung-sacs. Pocock (2) suggests that increased lightness would be an advantage derivable from this change; but Bernard (3, 7, 8) considers the tracheæ the more primitive, and the lung-sacs elaborated from them. He would derive the whole ventral series of arachnid breathing-organs, spinning-glands, coxal glands, and poison-glands from the ventral setiparous glands of an annelid ancestor. The tracheæ of insects, on the other hand, he believes to have arisen from the dorsal series of setiparous glands, and he finds the hairy area around the stigmata of the chrysalis of the Vapourer Moth

(*Orgyia antiqua*) recalls an annelid parapodium with its setæ. This supposed difference in origin of the breathing-tubes in the two classes leads to a supposed corresponding difference in the origin of the limbs, those of the insects being traced to the ventral series of parapodia of the annelid, and those of the arachnids (and also crustateans) to the dorsal series. Ingenious as this theory is, the great cleft which it makes between the insects and arachnids by destroying the homology of their appendages, seems a grave objection. The higher annelids have undergone too considerable a development along their own particular line to be safely regarded as approximating to the extinct ancestors of a distinct phylum.

However, in a recent memoir, Sinclair (9) also suggests the derivation of lung-sacs from tracheæ rather than that of the latter from the former. He describes peculiar breathing-organs in the Scutigeridæ, consisting of dorsal slits opening into air-sacs, from each of which a number of tubes, arranged in two semicircular masses, are given off; these organs are believed to represent a stage between ordinary tracheæ and the lung-books of spiders, the lungs of scorpions representing the highest development of the series.

But the relationship between the various kinds of breathing-organs depends upon the question whether the lung-bearing or tracheate orders of arachnids are the more primitive. In the scorpions, we find four pairs of lung-sacs, and the full segmentation of these animals has already been dwelt upon as evidence of their archaic nature. Moreover, while palæontology has not yet thrown much detailed light on the history of the arachnid groups, strong confirmation of this view is derived from the fact that remains of scorpions occur in the Silurian rocks, and that these are the oldest air-breathing animals of which we have certain knowledge. In the scorpion-spiders, and in the lower families of the true spiders, two pairs of lung-sacs occur, but in the higher spiders, as already mentioned, the hinder pair of these are replaced by tracheæ. Here, again, such knowledge of fossil spiders as we possess supports the view that lung-sacs preceded tracheæ, for while four-lunged spiders lived in Carboniferous times, the higher members of the group have only been found fossil in Tertiary formations. The comparatively recent orgin of the higher spiders is also suggested by a study of their distribution, most of the families and genera being world-wide in their range. Marx (10) has recently remarked that among 292 species of spiders from the Arctic regions, none can be referred to special genera; and British naturalists who have read Hudson's "Naturalist in La Plata" must have observed that, while the mammals and birds and insects described are far removed, indeed, from our native animals, in the chapter on spiders we are introduced to such familiar friends as *Tetragnatha*, *Zilla*, *Pholcus*, and *Argyroneta*. Among the four-lunged spiders we meet with the confined and discontinuous distributions so characteristic of old-time groups of

animals. For example, Simon (11) has recently described a new genus *Myrtale* from Madagascar, whose nearest affinities are with *Moggridgea* from South Africa, and *Migas* from New Zealand.

The remarkable group of marine arthropods known as the "sea-spiders" (Pycnogonida) were for long regarded as arachnids by some zoologists, and as crustaceans by others. Of late years, the great authority of Hoek and Dohrn has led to the relegation of these animals to a position independent of either group, indicating their supposed independent descent from vermiform ancestors. Recently, however, Morgan (12) has re-advanced the view of their arachnid affinities, dwelling on the similarity of the polar delamination observed in the division of their eggs to what occurs in the eggs of arachnids, and also on the presence of intestinal branches in the limbs, another arachnid character. The number of appendages is the great obstacle to bringing the pycnogons into line with the arachnids. Their four pairs of walking-legs, their chelate foremost jaws, and their palps are easily represented among arachnids, but what is to be done with the pair of extra limbs, behind the palps, on which the male—a true nursing father in this group—carries the eggs and young embryos? If we are to retain the pycnogons among the arachnids, we must either derive these extra limbs from a segment which has become aborted in the latter, or else, comparing them with the first pair of legs of a spider, regard the pycnogon's last pair of limbs as belonging to the abdomen.

The semi-parasitic life of pycnogons on hydroids, polyzoa, and other marine animals has always suggested that they are a degraded group, and degradation often masks affinity. In recent systematic work on pycnogons, Schimkewitsch (13) and the present writer (14) have directed attention to the relationships between the genera, and the former insists that the most primitive genera are those of most complex organisation. Not only do the number of appendages tend to lessen, the chelæ or palpi or both disappearing, but the number of joints in the appendages decreases and the beautiful and complicated serrate processes on the egg-bearing legs in the higher forms—the Nymphonidæ, for instance—degenerate into ordinary spines as we descend through the various series. There are few groups of animals in which a comparative study of adult living forms does not suggest the lines upon which progress has been made. In the pycnogons, however, as in most parasitic or semi-parasitic groups, such study can only tell us a tale of decline.

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Geo. H. Carpenter.

VIII.

The Great Barrier Reef of Australia.[1]

THE Great Barrier Coral Reef of Australia is 1,250 miles long, stretching along the coast of Queensland from Torres Straits to Lady Elliot Island. The distance from the outer edge of the reef to the mainland varies from 10 to about 150 miles. Already raw material to the value of £100,000 is obtained annually from the reefs and the intervening water, and is exported from the colony.

The chief items in this produce come from the pearl, and pearl-shell, and the Trepang (or Bêche-de-mer) fisheries. In addition to such commercial interest, the great Barrier Reef is of the greatest scientific interest from the material it provides for collecting information as to the external features and detailed composition of coral reefs.

Mr. Saville Kent, Commissioner of Fisheries to the Government of West Australia, was afforded the opportunity, by the wise liberality of the Queensland Government, of making a detailed examination of this great reef. This sumptuous volume contains the first results of his labours. He hopes to publish fuller and more technical accounts of various animals examined at a later date. The present volume is designed to give both the scientific and the general public an idea of the vast and wonderful medley of life presented by a coral reef.

An interesting and novel feature of the volume is its copious illustration by photo-mezzotype plates.[2] These provide for us a series of pictures of the coral reef of almost unimagined beauty and value. As the reefs are uncovered only for short intervals, and under conditions unfavourable for drawing, we have had as yet, except for the verbal descriptions of observers, no idea of the actual appearance of a reef. At the end of the volume there are sixteen chromo-lithographic plates. It is impossible to deny that these are, artistically, far from pleasing; but even if they are exact representations of the animals, it may well be that the glaring colours of the tropic seas removed from the brilliancy of tropic sun to the cold atmosphere of England seem harsh and crude. Moreover, the æsthetic value of

[1] THE GREAT BARRIER REEF OF AUSTRALIA; ITS PRODUCTS AND POTENTIALITIES. By W. Saville Kent, F.L.S., F.Z.S., F.I.Inst. With a Chart, 48 Photo-Mezzotype plates; 16 Chromo plates; and many Woodcuts. Pp. 380. 4to. London: W. H. Allen & Co., 1893. Price £4 4s.

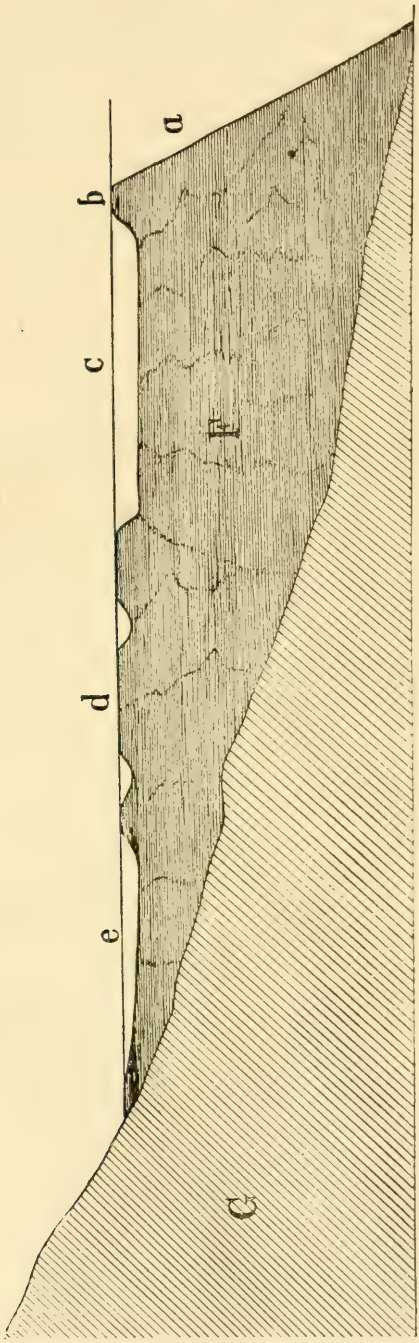
[2] See NATURAL SCIENCE, vol. i., p. 648.

colours is a mere convention—as anyone may observe who takes his pleasure in the Row this season.

Geologists and others will perhaps turn first to the section of the book which deals with the origin of coral reefs.

A figure which by the kindness of the publishers we have been able to reproduce shows how the reef forms an enormous buttress of rock F pressed against the sloping granite rocks G of the coast. Mr. Jukes, and after him the author, made most careful search for any evidence of upheaval, but failed to find facts against Darwin's subsidence hypothesis. At many points along the reef enormous masses of storm-raised coral at first sight simulate the effects of upheaval; but of exact evidence Mr. Saville Kent could find nothing to oppose seriously the subsidence theory. At many stations along the reef, large expanses of dead coral intervene between the living banks and high-water mark. Such banks are subjected to destructive atmospheric influences at every spring-tide, while living coral is above water only at the lowest spring-tides. Dead bivalve shells of large size such as *Tridacna* and *Pinna* occupy their original positions in close contiguity to the dead corals; but all these results could have been caused by upheaval of a foot or two, and there is practically no evidence for a prolonged era of upheaval.

On the other hand, all of the few big breaches in the Barrier's outer rampart are opposite large estuaries. They are at present too remote from the estuaries (from 30 to 60 or 80 miles) for the influence of fresh-water to have retarded coral growth. This condition is by itself strong evidence for subsidence, but Mr. Saville Kent relies most strongly on the evidence from the geographical distribution of animals that Australia had in former times a land-connection with New Guinea. It is not worth while to recapitulate the facts here, as all who are specially interested in the controversy will refer to Mr. Saville Kent's book; but he concludes that the construction of the Great Barrier of Australia under conditions of subsidence, and in accord with the original hypothesis of Mr. Darwin, is proved. Towards the end of Tertiary times the great Barrier Reef existed as a simple fringing reef, and moderate subsidence, of the occurrence of which we have abundant geological evidence, has turned it into its present condition. Mr. Saville Kent brings forward a considerable additional quantity of evidence supporting parts of the reef-formation theories of Murray and others. Although the Barrier Reef as a whole has been formed by subsidence, in the details of the process conditions of local growth and decay have played a large part. Thus, much of the actual bulk of a reef is formed of coral *débris*—*débris* which comes from the breaking up under the influence of storms of the rapidly-growing, more fragile forms of coral. On a reef at any time only a small area is actually alive, and that small area is turned most usually away from the mud and fresh-water on the landward-side, and turned towards the most abundant and freshest



DIAGRAMMATIC TRANSVERSE SECTION OF THE GREAT BARRIER REEF OF AUSTRALIA.

G.—shore of granite. F.—coral reef. *a*.—deep seaward face of reef. *b*.—actual barrier. *c*, *e*.—clear channels. *d*.—inner reefs.

(Vertical scale much exaggerated.)

supplies of sea-water. Again, nearly all masses of coral grow more or less like a fairy ring—living and expanding at the edges, decaying, getting ground down into *débris*, or dissolved by the sea-water at the centre. The action of various forms of life living on the coral also assists this process. Shell-fish grow on the coral and become embedded in it, and round the intruding giant clams embedded in the coral waves a region of decay is usually present.

A valuable part of the book is occupied by an account of the actual corals and coral animals. To those who are familiar only with the bleached and dried specimens of museums and cottage windows, the account of the marvellous colours of the living corals will be a revelation. The soft tissues of the polyps have all the colours of the rainbow—sometimes blended in soft and delicate shades, often in the most startling and violent mixtures. The other reef-living animals—animals like the star-fishes, sea-urchins, sea-cucumbers, crustacea, and shell-fish—which contribute to the lime-supply of the reef, and the various fishes living in the shallows, all are similarly radiantly attired.

Foraminifera are present in abundance, and their calcareous shells form the slimy mass of the white "sandy patches" that intervene between the coral banks.

The Nullipores or coralline seaweeds form encrusting masses over the reefs rather like Madrepores, but growing to a height considerably above the Madrepore localities. Those on the surface are chiefly pink or lilac, and so they give a characteristic appearance to the reefs they infest. Other Nullipores (*Halimeda*) grow in deep pools or on the sea-bottom, and are brilliantly grass-green.

Everywhere on the reef sea-anemones are found in abundance. Most striking are some giants belonging to the genus *Discosoma*. Some of these had discs of between one and two feet in diameter and acted the part of hosts to two species of fish and a species of prawn. In *Discosoma kenti* there were almost invariably one or two specimens of a percoid fish about three inches long. When a stick was thrust into the mouth of the anemone the fish swam out, but when the disturbance was over returned to the seclusion of the anemone's stomach. A prawn about two inches long had similar quarters. Both the fishes and the prawns were brilliantly coloured.

The tentacles of many of the reef-living anemones were conspicuous for length and branchings like the fronds of a fern. Many of the anemones, especially those with peculiar tentacles, have the power of stinging.

Among the corals themselves almost every variety was found, and Mr. Saville Kent has the materials for a large addition to recorded species and genera; but most interesting are his numerous descriptions and figures of the appearance and colours of the living expanded polyps. By an ingenious arrangement of his camera he took many photographs of the expanded corals either in their normal

positions in shallow parts, or when they had expanded in his tanks and tubs.

Among the Alcyonarians, the Alcyonidæ, from their number and abundance, are very conspicuous on the living reefs; but as their most abundant forms possess a flexible corallum with separated spicules of lime, they break up on death and add only to the general powdery *débris*.

Tubipora, the organ-pipe coral, is present for the most part not in large patches, but scattered about further inshore than ordinary corals, and in regions where there is much muddy sediment. *Heliopora cærulea* was found in abundance, and while the author now confirms Moseley's reference of the Heliopores to the Alcyonarians, he is inclined to believe that a part of the corallum is built up by commensal worms closely allied to the *Leucodore ciliata*, which did so much to destroy the Australian oyster-fisheries. The account in the book, however, would be equally consistent with a purely parasitic relation of the worm to the coral.

A new sheaf-shaped Alcyonarian, called *Xenia pulsitans* by the author, and found in Torres Strait, is remarkable not only for the size and delicate colouration of its polyps, but for certain special physiological manifestations. "The expanded tentacles in this type measure over an inch in diameter. The colour of the stalks and of the main shafts of the tentacles is a pale beryl-green, while the conspicuous tentacular pinnæ, and the substance of the common supporting polypary, are a pale ochreous-brown. The special physiological phenomenon observed of this type was associated with the movements of its tentacles. In all ordinary polyps, whether belonging to the coral-secreting or skeletonless sections, the component tentacles move quite independently of one another, and their action is either irregularly vermiculate, or one of simple expansion and retraction. In the present type, on the contrary, all the eight tentacles move synchronously, opening out and contracting in a continuous measured rhythm, averaging two seconds to each contraction. The action thus observed was in all respects identical with the pulsating contractions of a jelly-fish, and was suggestive of a less remote affinity between the Alcyonarian tribe and the medusiform Hydrozoa than subsists between the last-named tribe and the coral-forming Madreporaria or skeletonless Actinozoa. This suggestion of affinity receives substantial support from the fact that as the radial processes throughout the jelly-fish tribe are invariably a multiple of four, and most commonly eight, they thus correspond in number with the tentacular organs of an Alcyonarian."

Among the Hydrozoa the only generic type that contributes to any material extent to the formation of the reef is that of Millepora. The brilliant *Distichopora coccinea*, which resembles *Corallium rubrum* except that its skeleton is commercially valueless, is found not infrequently.

The pearl and pearl-shell fisheries are of great importance. As an export from Queensland, pearl shell has ranked for the last five or six years as the sixth or eighth most important substance. These fisheries are confined to the tropical area of the Queensland coast, and are intimately associated with the Barrier-reef. The mother-of-pearl shell is collected in water from 7 to 20 fathoms deep. Formerly it was found in areas practically exposed at low tides. Queensland derives a revenue from licenses required to be taken by boats, ships, and sailing masters, and from a small royalty on the amount of shell collected.

Owing to considerable depletion of the more readily accessible fishing grounds, the average weight of shells has decreased. Large shells weighing six to eight pounds a pair are still to be got, but the more ordinary yield averages two-and-a-half pounds the pair. A protecting Act of Parliament now prohibits the taking of shell with a less diameter than six inches across the pearl lining or "nacre."

The specific form of the Queensland waters is *Meleagrina margaretifera*. Actual pearls of large size and fine quality are found, but enormous numbers of the shells contain worthless forms.

Some experiments have been made in the artificial production of pearls, not according to the Chinese methods by introducing metallic or other foreign bodies into the mouth to be covered by a pearly coat, but the process has as yet only a speculative value.

The author made a series of experiments with a view of finding methods of removing young shells from the breeding grounds to shallow water where they might grow to maturity in undisturbed security. He found that young living examples with a diameter of no more than a quarter of an inch might be safely removed by cutting through the byssus. When the animal was placed in an aquarium the remains of the byssus were ejected and a new byssus secreted, and in most cases a secure reattachment formed in the new habitat. The animals have a slight power of locomotion, but nothing comparable with the active movements of pectens.

In removing larger shells from deep water for relaying in shallow water, several methods were tried. Some were placed in shady places on deck and had sea-water thrown over them at intervals. For others, the American method (employed in the case of oysters)—muzzling the shells with wire to prevent the opening of the valves—failed to secure the retention of the fluids in the shell, as they escaped by the byssus groove. The mortality in these two cases was very great, but entirely successful results were obtained when the shells were kept in tubs of water, the water of which was changed every few hours, and the actual shells immersed in basket-work frames in the sea at night.

When these shells were laid down in frames in shallow water where the water was pure and the currents strong, remarkably healthy and active growth took place. It may indeed be said that

Mr. Saville Kent has succeeded in demonstrating the practicability of pearl-shell farming.

An interesting and curious chapter deals with the Bêche-de-mer or sea-cucumber fisheries. The food of the sea-cucumbers consists almost entirely of foraminifera which are swallowed in combination with large quantities of shell and coral sand. So physiologically the sea-cucumbers are the earth-worms of the sea, and pass through their bodies immense quantities of material in process of getting the scanty food substances contained in it.

The animals are prepared entirely for the Chinese market. They are collected by hand in sacks at low water. Immediately on their arrival at the curing stations they are placed in large cauldrons of water and boiled for twenty minutes. They are then split open, gutted, partially dried, and finally smoked, the favourite wood being that of the red mangrove (*Rhizophora mucronata*). The prepared animals should be perfectly dry and crisp. Mr. Saville Kent does not, unfortunately, give further directions in the matter, though one would have liked to know the recipe for their final cooking, and any commendation or otherwise of their flavour.

Little seems to be known of the rate of growth of these Holothurians. A good many forms are present. The cotton-spinners have no commercial value ; many others from the soft texture of their tissues cannot be cured. The author sent a complete set of specimens to the British Museum, where they have been described by Professor Jeffrey Bell. *Holothuria mammifera*, called by the appetising Chinese name Se-ok-sum, appears from its market price to be the surest tickler of the Celestial palate, but *Actinopyga obesa*, under the name of Hung-hur, runs it close.

While the Bêche-de-mer fisheries have yielded an average of £23,000 a year, the Queensland oyster fisheries produce only about £8,000 a year. The oyster is *Ostrea glomerata*, the "rock oyster." Other forms are quite edible and wholesome, but have not yet secured a market. The oysters occur in the tropical waters, and so come into the Barrier Reef fauna, but the actual fisheries are, for the most part, south of the reef. *Ostrea glomerata* has a number of well-marked varieties. The shallow-water forms are the most typical, and have luxuriantly frilled and convoluted marginal borders, and bright colours. The deep-water varieties have a smoother and more ponderous form, often an abnormally elongated contour, and are much less conspicuously coloured.

The old travellers' tales are realised, and in the region of the Barrier Reef oysters are to be found growing on trees. That is to say, one of the most favoured habitats of the "rock oyster" is the exposed roots and respiratory shoots of the white mangrove (*Avicennia officinalis*.)

The oyster has many enemies. A small boring whelk (*Urosalpinix paviæ*) does immense damage, especially among the young oysters. Mr. Saville Kent seems doubtful as to whether or no the star-fish

deserves its evil reputation in this matter, but until its innocence be established he recommends its wholesale destruction.

The common sting-ray, *Trygon pastinaca*, is most destructive, and *Cestracion philippi*, the Port Jackson shark, makes use of its crushing teeth to very evil effect among the oysters. Boring sponges, birds, and various other forms of life prey on the oyster, but the most destructive pest is the small worm, *Leucodore ciliata*, whose ravages were first described by Mr. Whitelegge.

In a special chapter, entitled "Food and Fancy Fishes," the author deals with some of the 900 recorded species of Queensland fish. Among food-fishes, those of the Northern district belong to the Indo-pacific or Oriental region. The trumpeters and barracutas, which are so important in the fish-markets of New Zealand, are practically unrepresented in Queensland. The best known forms are *Lates calcarifer* (the "Cockup" of the Calcutta market), and the smaller *Lates colonorum*. Mullets, gurnards, sea-pikes, flat-fish, and many well-known forms abound. Eight species of herring (*Clupea*) have been recorded, but, as yet, have not been utilised for commercial purposes. Altogether, the "unvintageable sea" seems to be remarkably prolific, and Mr. Saville Kent makes out a good case for regarding the Barrier Reef as one of the most remarkable commercial advantages of Queensland. In a concluding chapter entitled "Potentialities," he points out how, under proper scientific advice, this commercial advantage admits of almost indefinite extension. As the population of Australia increases, the value of so large an area of prolific sea-life will become almost inestimable, and, quite apart from the scientific value of Mr. Saville Kent's beautiful volume, Queensland is to be congratulated on taking so efficient means as the assistance of this work to develop its resources. On the side of practical utility it were hard to see how Mr. Saville Kent could have done better work. He has surveyed the actual resources of the reef and has shown the small use that is at present made of them, bringing to the task not only a trained scientific mind, but a large special experience.

To those who spend their days in laboratories and museums, and who will eagerly turn to the separate scientific accounts that have been, and will be, published on Mr. Saville Kent's collections, this work is equally invaluable; for here are the animals, not as specimens with names new or old, hinging on some obscure point in anatomy or doubtful question of priority, but the animals alive, in their actual places in the world, in their crowded environment of friend and foe, of sunlight and waters. In this short sketch of the book very little has been said of its most valuable scientific feature—the faithful and minute account it gives of the actual appearances presented by a living coral reef; but as the value of this account is inseparably associated with the beautiful photographic plates, those interested must be referred to the book itself.

SOME NEW BOOKS.

THE GERM-PLASM: A Theory of Heredity. By August Weismann. Translated by W. Newton Parker, Ph.D., and Harriet Rönnfeldt, B.Sc. Contemporary Science Series. Pp. 477. With 24 illustrations. London: Walter Scott, 1893. Price 6s.

THE new volume of Professor Weismann, of which the translation is before us, departs from the historical method in which we have received his work. This is a deliberate presentation of his theory, incorporating, expanding, and explaining the ideas which have been shaped in his various essays. At the same time, it contains a number of novel hypotheses subsidiary to the theory, but necessary to it. It is not too little to say that the book is an excessively hard nut to crack, and that many will declare the kernel bitter who have not bitten through the epicarp.

Speaking generally, this expanded account is a closer and more detailed theory of the structure and mechanism of the germ-plasm, bringing it closer into relation with, on the one hand, recent advances in actual observation of the structures and changes in the nucleus connected with cell-division, and on the other, with the observed details of inheritance, variation, asexual reproduction and regeneration of tissues.

As before, the doctrine of the continuity of the germ-plasm is firmly insisted on, and it may be said that this, of all parts of Professor Weismann's theory, has been subjected to the least successful criticism, and has received the most startling corroboration. This germ-plasma Weismann identifies with the chromatin fibril in the nucleus of egg-cells and sperm-cells. Before fertilisation, the nuclear fibril breaks up into a series of loops which Weismann calls idants. In ordinary sexual eggs the number of idants is twice halved, and by what Weismann calls these two reducing divisions the polar bodies are extended. It will be remembered that, originally, the first polar body was considered by Weismann to serve for the extrusion of that part of the nuclear matter which, having served to guide the maturation of the ovum, became useless when the ovum was mature. In his completed doctrine, he finds in the two polar bodies of normal sexual cells, and in the single polar body of parthenogenetic ova, a mechanism for reducing the bulk of germ-plasm—part of the apparatus for phylogenetic variation. The actual nuclear matter which controlled the maturation of the ovum he supposes to have passed into the protoplasm of the ovum, and, therefore, to be out of the reckoning when the behaviour of the nucleus is being interpreted.

When the nuclear loops or idants are closely examined they exhibit a series of lumps or divisions as if they were built up of separate pieces like draught counters strung together. These sepa-

rate pieces Weismann names "*ids*," and suggests that the facts of inheritance might be accounted for if each id contained the possibility of producing the individual. On such a hypothesis Weismann shows most ingeniously and convincingly how the observed marshalling and sorting, division and rejection of idants and ids would form a mechanical apparatus compatible with the complex phenomena of inheritance to be accounted for. The next stage of his theory still is along the path of observation. A certain number of idants with the contained ids remain unused in each individual development. Occasionally, but rarely, they are passed at once into a cell which becomes the future mother sperm or mother ovum cell, and so the sperm-cells and egg-cells of the individual arise directly from the idants present in the fertilised egg-cell. More frequently, however, the set of idants and ids are handed on passively from cell to cell during the development of the individual, until they ultimately reach the place in the individual where the sexual cells are to be formed. This path of the germ-plasm he calls the "germ-track," and, as will be remembered, it was his original hitting upon such germ-tracks in the Hydrozoa that led him to suspect the existence of a germ-plasm. Turning again to that part of the germ-plasm which is to form the actual individual, we come to entirely theoretical matter. It consists of *ids*, and each *id* contains the possibility of a complete individual. The *id* is not a structureless substance, but possesses a definite historical architecture—the expression of the past history of the species. Each *id* is built up of smaller units, the "determinants," and there is a "determinant" for each cell or group of cells in the animal or plant body which can vary independently. As development goes on the *ids* gradually break down, throwing off in the order determined by their historical architecture the determinants to rule the structure of the "determinates" or independently varying parts of the organism. Thus ultimately we come to determinants passed during cell-division into their appropriate cells. In these cells the determinants break down into the final units or "biophors" which pass through the nuclear membrane into the protoplasm of the cell. Thus the "idioplasm" or chromatin derivative which rules the cell consists of Weismann's theoretical determinants, and it rules the cell by the passage of actual material particles into the protoplasm. These biophors—the ultimate units of the germ-plasm—are, at the same time, the primitive units of life, and Weismann conceives life as having originally consisted of independent biophors.

For each of the units of the germ-plasm Weismann postulates the common property of living material, the power of growth and reproduction by division. This has of course been observed in "idants" and is a ready inference for "ids."

It is to be noted that a number of ids, each with the complete power of producing an individual, are present in the development of an individual. Thus there comes about the struggle between the determinants and biophors coming from ids with slightly different peculiarities, and thus the particulate variability of the parts of an organism and the particulate resemblances to parents and ancestors have a possible material apparatus.

As the whole volume consists of a subtle and ingenious translation of biological phenomena into the terms of this theoretical apparatus, it is obvious that only a study of the book can give an idea of the exactitude with which the phenomena correspond with the apparatus; and for this reason a sketch like the above removed from the com-

plexity of the observed phenomena of which it is the abstraction necessarily will seem artificial. To those who are inclined as they read these lines to cry out on the artificiality, I can only say—work through Weismann's application of his theory and see what a master key it seems to the confused tangle of life.

While at first sight this theory seems to present only a formal simplification of the facts of heredity, inasmuch as it seems to put on the biophor the whole mystery of life, I am inclined to think that this objection is not valid; for if the mechanical marshallings and evolutions and devolutions of the biophor and its higher aggregations do actually form a picture in little of the larger complexities of the visible world, we can at once remove from the problems to be solved these actual complexities. We shall, in fact, by Weismann's help, effect a separation of those problems of living material which are functions of the complexity of organisms and not actual difficulties of living matter itself; and those who are familiar with the recent advance in our knowledge of the elaborate marshallings and evolutions and devolutions of the chromatin in dividing nuclei, can hardly say that Weismann's postulated movements of the elements of the germ-plasm make a large demand on credulity; but this depends upon our being able to see clearly that the mechanism is a possible one. I freely admit that as yet I have not assimilated Weismann's book sufficiently to follow the clear image that is obviously present in his mind. He expressly states that he has felt the difficulties in giving up an epigenetic view of ontogeny.

It will be seen that, leaving out the foregoing considerations, we are led up to the ultimate problem in the biophor. That is really the problem of assimilation, and is the factor as yet unexplained in any theory of biology. The possible migration of biophors from the nucleus to the protoplasm usefully shifts the problem of "fernwirkung" (the translator rather unhappily renders this "emitted influence," instead of the obvious "action at a distance") to the protoplasm itself. When we understand how protoplasm can feed and grow, we shall probably have little trouble in understanding the nucleus.

P. C. M.

THE MICROSCOPE: Its Construction and Management. Including Technique, Photo-Micrography, and the Past and Future of the Microscope. By Dr. Henri Van Heurck. English Edition. Translated by Wynne E. Baxter, F.R.M.S., F.G.S. With plates, and upwards of 250 illustrations. London and New York: Crosby Lockwood & Son, 1893.

THE English microscopist has, for at least a generation, shown that he is sufficiently in earnest to be cosmopolitan. He can welcome warmly good English work, in either the manufacture or the use of the microscope, or in the excellence of a treatise on its principles and application. But there is definite evidence that he has learned to prefer the quality of an instrument or a handbook to its source. In fact, there is danger that the foreign maker, having obtained by sheer excellence and moderate prices an assured position in this country as the producer of the principal optical elements of the microscope, that the prejudice runs wholly in favour of instruments of foreign production, to the sacrifice of much that is of really higher excellence made in this country.

In the matter of handbooks, however, there is probably no country that has met the wants of the beginner and the skilled amateur so fully as England. Carpenter's "Revelations of the Microscope" has, in each of its seven editions, held its own not only in England and America, but has been largely used on the Continent. Nevertheless, and, perhaps, indeed for this very reason, there is no country in the world where the great Diffraction theory of Abbe, explaining the principles of vision with the modern microscope, has been so critically and, at the same time, so warmly and generally received as in England. At the same time, there are few working microscopists whose shelves are without *Handbuch der Allgemeinen Mikroskopie* by Dr. Leopold Dippel; and it may be safely predicted that the splendid *Theorie der Optischen Instrumente nach Abbe* by Dr. Siegfried Czapski, which has just come from the publishers' hands in Breslau, will anywhere find a more appreciative circle of students than in England; but the English microscopist, who is not only a student of what the microscope does and can reveal, but also a critic and connoisseur of the principles and modes of manufacture of the instrument, really needs books of the class above indicated. The day for handbooks that are better illustrations of the art of the printer and the wood engraver than of the science and art of microscopy has passed away.

In the treatise now before us great pains has been taken to present in its most attractive form the facts of microscopical science as known to us ten or fifteen years ago, and to make an appendage to these of the most recent knowledge which has come to us as the result of theory and practice in more recent times; but between the old and the new there is no coherence. The treatise purports to cover all the area of modern microscopy, in the interests and for the benefit of the amateur. That it fails of its purpose there can be no doubt. It does this partly because of its method. It adopts the older style of teaching, and simply forces the new optical doctrines and details of the new methods of manufacture and manipulation (where given at all) into a place or position in the text without preface or explanation. The result is that we obtain an account of the "Theory of Microscopic Vision" which is in itself excellent, but quite beyond the range of the class of reader for whom the book is ostensibly written, because, clear as Abbe is in the exposition of his great theory, it needs at least a chapter of explanation and introduction to make it fully accessible to the ordinary, and especially the non-mathematical, reader. The result is, that this book appears to us to fail from the fact that it directs the student to go beyond his depth, without even the semblance of assistance on the one hand, and, on the other, guides him with great care over shallows where no guide was needed, save the catalogues of certain English and foreign opticians—that is, as to the forms of certain instruments mostly on the same type.

It is quite true that the tyro wants above all things to know how wisely to purchase an instrument; but this is not unfolded to him by an indiscriminating description of the microscopes of certain makers however diverse, but by an unbiased description of the *essentials* of *any* good instrument, and such an account of practical tests as would enable a beginner at least to see how far a certain pattern of stand, or a specific instance of workmanship, answers to this set of requirements. And in furnishing these data there need be no more dogmatism than in an experienced photographer's affirmation

of what is indispensable in a thoroughly good and practical camera — always in both cases keeping the specific object which is sought in view.

It is true that Dr. Van Heurck has presented us with a very attractive model of a microscope for "the study and photography of Diatoms, and all delicate researches," but the model has not many new or specially advantageous features when considered as one of the many English models before the public. It has, as we think, in common with some of the very finest English workers, some distinct disadvantages; but it certainly overcomes the difficulty of the working of the fine adjustment on good conditions better than any instrument that we know of the Jackson model (which is in practice an inferior form) that adopts similar methods. The fine adjustment is delicate, and the instrument is throughout supplied with admirable facilities for accuracy and ease in use. Hundreds of skilled workers would refuse it at once as representing the best possible form of a modern microscope; but much has been done to soften the evils inherent in the form. Yet all these are costly, and when they are effected we are bound to submit that it is not the best form of the instrument that is either possible or accessible; and, above all, it is not the best form of the instrument that can be constructed at a low expenditure.

The amateur wants a useful, well-made instrument at a low cost. So does the medical student. What, therefore, is essential to a good microscope in any form? What form of stand has the largest number of points in its favour, practically considered? and which are the stands of the modern makers, English, Continental, or American, that, at a moderate cost, most largely meet these requirements?—these are the considerations which are of the greatest value to the purchasers of modern handbooks, from this side of the subject, and its efficient discussion would involve a consideration of that most practical of all questions in such a treatise, the relative values of the English as against the Continental stands, or *vice versâ*; but this is wholly avoided.

There is one other point which we desire to touch but lightly, it is that here and there in the book phraseology has been adopted that certainly does not represent, if it does not absolutely run counter to, the diffraction theory of microscopic vision. It is not necessary to dwell on this; probably it may in some instances be inadvertence; but in a treatise intended to teach *ab initio* the doctrines of Abbe on the optics of the modern microscope, especially as there is some obscurity in the inculcation of these, it is unfortunate to find incidental passages that imply not only complications but contradictions.

The translator's work has manifestly been done with care and sincerity, and most fairly presents the meaning of the author; and the book, though large and heavy for use at the work-table, is unusually good.

There are many points of excellence in the work, and some that give evidence of the practical skill of the author; indeed we can see the possibility in a second and greatly revised edition of a book of high quality and usefulness; but it will involve the excision of much that is now useless in its pages, and the rendering useful by ample and lucid preface and explanation of much in it that, although useful in itself, is doubtfully, if at all, useful in the form and relations in which it is presented to the enquirer in the book as it now stands.

THE GLACIAL NIGHTMARE AND THE FLOOD. A second appeal to common sense from the extravagance of some recent geology. By Sir Henry H. Howorth, K.C.I.E., M.P., F.G.S. 2 vols. 8vo. Pp. xxvii and 920. London: Sampson Low & Co., 1893. Price 30s.

SIR HENRY HOWORTH, in the two volumes which now lie before us, stands forth as the champion of an almost universal deluge. It is not, he is careful to explain, the deluge of Noah; but it appears to be a considerably worse one, which exterminated the mammoth and could transport erratic blocks as large as houses across country for distances of many leagues. In fact the book is a nine-hundred page supplement to his bulky volume on the Mammoth and the Flood, and is to be followed by still another one. It is not altogether easy to understand the reason of the appearance of this mass of undigested extracts. The new book seems to be intended as a sort of homeopathic remedy for the "Glacial Nightmare"; but, unlike most homeopaths, the author, instead of using a small dose, attempts to cure by the exhibition of a far more serious incubus than the one we are suffering from.

The method adopted in these volumes is to put together under several heads extracts from various opinions as to the mode of formation of the Drift or Diluvium. We thus find in the first three chapters quotations from writers between 1719 and 1840, who referred the transportation of erratic blocks to the agency of water. Then follow chapters on the champions of icebergs, and of glaciers, and two more on the "growth and culmination of the glacial nightmare." We read next a chapter on the "alleged recurrence of glacial epochs and on supposed inter-glacial beds," followed by others entitled "appeals to transcendental physics and astronomy" and "meteorology." Chapters XI. to XVII. represent, apparently, the views in favour with the author, for they contain selected extracts from the evidence of such witnesses as could testify in any degree against recurrent ice ages, against a glacial period in the southern hemisphere, against the power of ice to do anything in particular, and against the occurrence at any period in regions now temperate of accumulations of ice other than large glaciers. Chapter XVIII., the author's own, is entitled "The distribution of the drift can only be explained by invoking a great diluvial catastrophe."

Sir Henry Howorth's volumes will be useful as giving references to old and forgotten writers on diluvial theories, and we have ourselves noticed several that were new to us. The more modern portions, and those relating to glaciation, are most imperfect. The Scandinavian writers who have done so much to increase our knowledge, are, for instance, almost ignored, or only quoted at second-hand from the "The Great Ice Age"; even the English and American literature of recent years has not been properly examined. After going through the two volumes carefully, and noting how time after time the author brings forward untrustworthy witnesses in favour of his own case, and omits to call the strongest on the other side, we cannot recommend the "Glacial Nightmare" to the student as giving either an accurate or impartial summary of the present state of our knowledge. We observe that the volumes are marred also by an enormous number of errors and misprints. Only ninety of these are corrected in the author's own "table of errata," but ten of the corrections are themselves wrong! Where quotations have had their punctuation or wording altered, inverted commas should not be used; and we still less like to see them inclosing inaccurate translations

from foreign memoirs. We do not intend to imply that there is any wilful distortion of the quotations, for the errors are just as numerous in those in favour of Sir Henry Howorth's own views as in those from opponents. It is merely another instance of the carelessness so conspicuous throughout the book.

Gun and Camera in Southern Africa: A Year of Wanderings in Bechuanaland, the Kalahari Desert, and the Lake River Country, Ngamiland. With notes on Colonisation, Natives, Natural History, and Sport. By H. A. Bryden. 8vo. Pp. xiv. and 544. Illustrated. London: Stanford, 1893. Price 15s.

South and East Africa being at the present time on the "boom," the reading public is almost overwhelmed with the number of works appearing in rapid succession on that country, its peoples, and its products. Most of these, unfortunately, have little interest for the

Head of Lechée Antelope (*Cobus lechée*)

naturalist, who too often cannot but regret that the lack of suitable training has rendered so many of the pioneers of civilisation unfitted for giving any account of the animals with which they meet. This, however, is not the case with Mr. H. A. Bryden, who has already made himself known as an observer of wild animals in their haunts, in his work "Kloof and Karroo in Cape Colony," as well as in various articles contributed to the *Field* and *Land and Water*. The present volume naturally contains a large amount of matter which is chiefly interesting to those desirous of obtaining information as to the nature of the country and its development; but there are several chapters which cannot fail to be profitable reading to the zoologist. Among these we may especially mention those bearing the titles "Natural History Notes," "The Giraffe at Home," "The Waterway and Water-fowl of the Botletli," "The Game Birds of Bechuanaland," and the "Present

Distribution of the Large Game of Bechuanaland, Ngamiland, and the Kalahari."

In the Livingstonian epoch, every book on Southern Central Africa contained accounts either of new mammals or of the countless swarms in which species previously known had been met with. Alas! these days are over; and Mr. Bryden's account of the mammals of the districts he visited is, to a great extent, one continued lament on their decadence or disappearance. Thus he speaks of hartebeest surviving only on one protected farm, and records the disappearance of the last brindled wildebeest from the districts he visited; while he confirms the general opinion as to the total extinction of the quagga. Perhaps the most interesting chapter in the whole book is that relating to the giraffe, whose last haunts in Southern Africa are the thirsty regions of the northern Kalahari and Khama's country. The author dwells on the imperfect idea we obtain of this magnificent ruminant from the dwarfed and pallid specimens seen in menageries; and urges on our museum authorities how important it is that they should obtain the skin of an old chestnut bull before it is for ever too late. He mentions that in Khama's country the giraffe is only safe during the lifetime of the present chief, but urges that one of the English companies should, when the time arrives, take up its protection. In its last native stronghold, in the northern Kalahari, the author states that there is a prospect of water being obtained (although he does not say how), and if this should be the case, and protection afforded in Khama's country be withheld, then good-bye to the giraffe in Southern Africa. Surely, under these circumstances, our Zoological Society ought to spare no expense in endeavouring to procure a pair of these animals; and as money—if only there be enough—will do most things, it ought to effect this. We may add that Mr. Bryden is convinced that, in the Kalahari, the giraffe, in common with several antelopes, never drinks.

We have not space to refer to the author's observations on other mammals and birds; but we may point to him that if he considers it necessary to add the scientific names of well-known animals—a custom we consider perfectly superfluous in a popular work, and only irritating to the reader—he might take care to use the proper ones. *Oryx capensis*, for instance, is not the correct title for the gemsbok, neither should the secretary-vulture be alluded to as *Sagittarius secretarius*. Then, again, although we are aware that it is rash to say among what strange associates any particular bird may *not* have been placed by modern systematists (!), yet we scarcely think the statement (p. 350) that jacunas "are usually placed by naturalists in the family of Palamedeidæ, or screamers, between the snipes and rails" can be accepted as up to date.

On the whole, however, the book is, for an amateur naturalist, remarkably free from zoological blunders; and while its pleasant style and beautiful illustrations cannot fail to render it attractive to the general reader, it contains many observations on the habits and distribution of South African animals which must give it a considerable amount of value to the zoological student. R. L.

THE NESTS AND EGGS OF BRITISH BIRDS, WHEN AND WHERE TO FIND THEM; being a Handbook of the Oology of the British Islands. By Charles Dixon. 12mo. Pp. xii. and 371. London: Chapman & Hall, 1893. Price 5s.

MR. DIXON is a prolific writer on British birds and their ways. Only recently we had to notice a respectably-sized volume on the Game-

Birds and Water-fowl from his pen, and now we have the smaller work before us. In his preface, the author tells us that the idea of the work occurred to him about a dozen years ago, since which date he has been assiduously collecting material, and, accordingly, with the numerous other works on the subject for his guidance, it ought to be as near perfect as possible. It might be thought that by this time the subject of British birds was well-nigh exhausted, but as Mr. Dixon's work treats solely of those species nesting in the British Islands, and is mainly confined to their nesting habits, it will doubtless fill a void and command a ready sale, more especially as the larger works of Morris and Seebohm, treating more or less specially of eggs and nests, are too expensive for many purses. Like all Mr. Dixon's productions, the work before us is well and pleasantly written, and the amateur naturalist, as well as every young person interested in this fascinating study, cannot do better than forthwith provide him- or herself with a copy.

Of course we miss illustrations, but as figures of eggs are not of much use unless coloured, the cost of plates would have made the book so expensive as to have defeated one of the objects of its production, and the student must accordingly do the best he can without their aid. In regard to classification, the author may be ranged among the "lumpers," seeing that he puts all the non-diving ducks in the genus *Anas*, and employs the genera *Charadrius*, *Totanus*, *Scolopax*, etc., in a wide sense. Considering, however, that there is but an interval of some two months between the date of publication of the present work and the one on Game-Birds, it seems rather a pity that he could not have definitely made up his mind what names he was going to employ, as it is rather puzzling to the beginner to find the Dotterel alluded to in one work as *Charadrius morinellus*, and in the other as *Eudromias morinellus*. Moreover, a little more attention to the index at the end of the volume would have been desirable, seeing that the word "snipe" is omitted therefrom. The mention of snipe reminds us that we consider Mr. Dixon to be wrong in persistently stating that the common species is not gregarious, although we are fully aware that the individuals do not mass together after the manner of ruffs and plovers. No one who has shot in a Bengal "jhil," with snipe rising as thick as flies all round him, can ever possibly think of these birds as being anything else but gregarious.

All these points are, however, but trifling blemishes in a volume which leaves nothing to desire in the way of "get-up," and fully deserves all the success we can wish it. R. L.

The Geology and Palæontology of Queensland and New Guinea, with 68 plates and a geological map of Queensland. By Robert L. Jack, F.G.S., F.R.G.S., Government Geologist for Queensland, and Robert Etheridge, Junior, Government Palæontologist (New South Wales). 4to, Pp. xxx. and 768. Brisbane: James Charles Beal. London: Dulau & Co., 1892. Price £2 2s.

In Europe we scarcely realise how much good scientific work is being done in our Australian colonies. Geology in particular is well studied, for the prosperity of these colonies is so largely dependent on their mineral resources, and it is so essential in an arid region to understand where water can be obtained by artesian borings, that considerable sums are willingly devoted to the making of careful

geological surveys. Queensland, though behind certain of the other colonies, has now made the very handsome contribution to geological literature which lies before us, published under the authority of the Minister of Mines.

Mr. Jack, after ten years' experience on the Geological Survey of Scotland, was appointed Geologist for Northern Queensland in 1877, and since that time has been unravelling the geology of the country and studying its minerals and mines. A number of isolated reports have already been published by himself and his assistants, and materials for the present work have been accumulating for the last fifteen years. In 1881 Messrs. Jack and Etheridge, Junr., determined to combine their labours, and published an Australian Geological Bibliography. Then followed a handbook explanatory of the exhibits in the Colonial Exhibition of 1886, which in some measure led up to the volumes which have just appeared.

So great a mass of detail and so many subjects are treated of that it is impossible critically to review the book, and we can only give an outline of its contents. Speaking generally, for the stratigraphical and mining sections, Mr. R. L. Jack is responsible, while Mr. R. Etheridge, Junr., has undertaken the palæontology. The method of arrangement adopted is mainly stratigraphical, each series of rocks, beginning with the oldest, occupying a separate section. The minerals and fossils are treated of according to the age of the deposits that contain them, instead of being relegated to separate appendices, as is perhaps more usual. In many respects this arrangement is the most convenient, but it is not altogether satisfactory, for in the absence of any subject index it is very difficult to find the references to particular minerals, unless one already knows the age of the formations in which they occur. Persons, places, and fossils are well indexed.

The oldest rocks in which fossils have yet been found in Queensland are the Middle Devonian, though below these occur various slates and schists of unknown age. Then follow unconformably the Permo-Carboniferous, Trias-Jura, and Upper and Lower Cretaceous. Between the Upper Cretaceous and the Lower Volcanic and Drifts, here doubtfully classed as Miocene, there is a wide gap, and the real age of the Tertiary rocks of Queensland is by no means settled, for fossil evidence is absent. The curious deposit of auriferous sinter at the celebrated Mount Morgan Gold Mine is considered by Mr. Jack to be of Tertiary date, though the evidence is not conclusive.

The section on Post-Tertiary rocks will be one of the most interesting to the European geologist, for in these rocks occur the remains of the wonderful extinct marsupials of Australia, and of numerous extinct birds. Discussing the question of glaciation in Australia, Mr. Jack writes: "No evidence of a Post-Tertiary Glacial Period has ever, so far as I am aware, been met with in Queensland, unless the presence of temperate plants on some of our tropical mountains be taken to afford the necessary proof." Further on, however, he mentions a recent visit to the celebrated glaciated rocks near Adelaide, in company with Professor Tate. Mr. Jack, after ten years' study of glaciation in Scotland, is a far better authority than most of the geologists who have discussed the matter, often without going to the spot, and we are interested to learn that he "came to the conclusion that Professor Tate's observation was correct in every particular, and, in addition, satisfied [himself] that the movement of the ice must have been from south to north."

The morainic *débris* has travelled from a spot forty-five miles to the south.

We have already remarked on the great importance to Queensland of its mineral wealth, and readers will not be surprised to find that a large part of this monograph relates to mines and mining. Indeed, we are somewhat surprised to see that the Government of a young country like Queensland has been so far-seeing as to understand the necessity of studying also the scientific aspects of the subject, and has gone to the expense of publishing so good a series of plates of fossils. This is as it should be, for even looked at from a purely economic point of view, it is most essential that the relations of the different deposits should be thoroughly understood, and their fossils ascertained, for the suitability of large areas for habitation depends mainly on the supply of water from artesian wells; other barren districts may be made profitable by the discovery of new mineral resources. One has, of course, no right to expect completeness at this early stage, and we think that Mr. Jack shows a thorough appreciation of the necessities of the case when he writes in his preface that "The highest function of a Geological Survey is to lay a basis for future scientific observations by accurately mapping the relations of the various formations met with in a given district." He "cannot say that this beau ideal has been reached in Queensland. In every country, and especially in every new country, it becomes necessary in the first place to give attention to districts remarkable for the presence or prospects of mineral deposits."

Les Alpes Françaises; la flore et la faune, le rôle de l'homme dans les Alpes, la transhumance. By A. Falsan. 8vo. Pp. viii. and 356. With 77 figures in the text. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

We have already noticed the first part of M. Falsan's work on the French Alps, namely, that dealing with the mountains, streams, glaciers, and meteorology, or the inorganic phenomena. The second part, now before us, treats of the animal and plant-life prevalent to-day or in past ages, as well as the part played by man. M. Falsan has been fortunate in securing efficient helpers for the special sections of his book. Thus, the Marquis de Saporta is responsible for the Palæobotany, which forms the subject of the second chapter, while, at the end of Chapter I., a general introduction by the author to the ancient Alpine flora, and its relation to the present one, the Marquis and M. Marion explain their theory "sur l'origine montagnard de la flore des Alpes."

Dr. Magnin follows, in Chapter III., with a succinct account of the vegetation of to-day, discussing, first, the influence of altitudes and the consequent modifications of species; secondly, the zones of vegetation, of which he makes four, viz.: (1) The Préalpes, or western outer Alps; (2) the granitic central Alps; (3) the south-west Alps; (4) the maritime Alps. In each case are mentioned the plants especially characteristic of the zones, and their geographical or altitudinal subdivisions. Next he briefly refers to the influence of the aspect, whether north or south, S.E. or N.W., and the nature of the soil as regards the marked difference between the vegetation of the silicious and calcareous districts, and finally discusses its relation with neighbouring regions, the central and eastern Alps, the central plain and the Pyrenees.

M. Falsan himself deals with the stratigraphical palæontology and the ancient fauna in Chapter IV., and in the succeeding chapter with the modern vertebrate zoology; while Chapter VI., devoted to the insects and molluscs, is the work of MM. C. Rey, C. Chantre, Falsan, and Locard. The Marquis de Saporta, in the next chapter, tells of the appearance of man in the Alps, his origin, and early immigrations, and traces his history upwards through the various ages to historic times, and finally, in the last, with the aid of M. Charles de Ribbe, gives an interesting historic account of his yearly transmigration with his flocks and herds, its disastrous effects, the efforts to combat the evil, dating from the Middle Ages, and the results of modern legislation. The illustrations are generally good, some of the fishes (by Leblanc) extremely so, and the publishers are to be congratulated on this valuable addition to their Contemporary Science Library.

THE FUTURE OF BRITISH AGRICULTURE; How farmers may best be benefited. By Professor Sheldon. Small 8vo. Pp. 158. London: W. H. Allen & Co., 1893. Price 2s. 6d.

In a series of eight chapters the author discusses some points of very vital interest to the farmer and the nation at large. The answer to the query "Will grain raising pay?" depends on whether the farmer can form a sufficiently powerful and intelligent combination to secure a fair arrangement in the relation of landlord and tenant. Protection "and that new economic craze 'Bi-metallism'" are ruled out of court. To place a duty on food imports would merely subsidise farming interests to the detriment of the rest of our industries. If we protect any we must protect all, and then we shall be no more forward. Once we were a wheat-exporting people, but since 1868 the acreage of wheat growth has fallen from nearly 4 millions to less than $2\frac{1}{3}$ millions in 1892. Canada, with its scores of millions of acres in the north-west and a favourable climate, will be the chief wheat-exporting country of the future, while mixed farming, stock, grass, grain, root, and green crops will be our mainstay.

One chapter is devoted to "the Beef of the future," another to "our breeds of sheep and how to mend them," and three to dairy farming, all of which contain valuable hints. Professor Sheldon speaks with authority, his book is well written, and promises to bring those who will read it more in touch with the agriculturist, his ways, and his difficulties.

CATALOGUE OF THE SNAKES IN THE BRITISH MUSEUM (NATURAL HISTORY). Vol. I. By G. A. Boulenger. London: Trustees of the British Museum, 1893. Price £1 1s.

THE first volume of this Catalogue comprises the families of Typhlopidæ, Glauconiidæ, Boidæ, Ilysiidæ, Uropeltidæ, Xenopeltidæ, and Colubridæ Aglyphæ (part), and contains descriptions of 523 species. The classification adopted is nearly similar to that formulated by Mr. Boulenger in his earlier work on the Reptilia and Batrachia of British India (1890), only some slight changes having been made in the arrangement of the genera. The Boidæ, and more especially the pythons, are regarded as the most primitive snakes; and the unnatural character of the old classification of the Ophidia into the

harmless and poisonous groups is particularly emphasised. A figure of the skull, with all the bones lettered, accompanies the definition of each family, and there are twenty-eight plates devoted to external form.

Au Bord de la Mer. By E. Trouessart. [Bibliothèque Scientifique Contemporaine.] 16mo. Pp. 344, figs. 149. Paris: J. B. Baillière et Fils, 1893. Price 3fr. 50c.

This is a handbook to the Geology, Fauna, and Flora of the coast of France from Dunkerque to Biarritz, and will be of considerable service to those who wander along these shores. The volume begins with a sketch of the geology, special reference being made to the cliffs, and, after touching on the littoral zones, proceeds to discuss in order, from the Algæ onwards, the life to be met with in the waters. It should form part of the outfit not only of naturalists, but also of those who desire a casual acquaintance of the animals and plants they meet with during a seaside holiday.

An Analytical Index to the Works of the late John Gould, F.R.S. With a Biographical Memoir and Portrait. By R. Bowdler Sharpe, LL.D. 4to. Pp. xlviii., 375. London: Henry Sotheran & Co., 1893. Price £1 16s. net.

This is a valuable index containing nearly seventeen thousand carefully verified references to the genera and species of birds and mammals described in the works and memoirs of Mr. Gould. Both the scientific and the English names are quoted, and a number of extra synonyms are added from the more recent monographs of other authors, which in a few years will have familiarised naturalists with a certain set of names not occurring in Gould's works, though the species may be duly figured therein. The Biographical Memoir is accompanied by an excellent photographic portrait, while the Index is prefaced by a detailed list of Mr. Gould's 18 folio works and 300 small papers. We congratulate Dr. Sharpe on the successful completion of his laborious task, for which ornithologists owe him a debt of gratitude. The volume is beautifully printed, and a large-paper edition has also been prepared.

Curtice's Index and Register of Periodical Literature. Weekly. No. 1, vol. i., April 3 to April 8, 1893. Price 6d.

This interesting new Index, which proposes to contain references to important subjects in daily newspapers, weekly, monthly, and quarterly publications, will be an invaluable help to all who are anxious to keep posted up in special subjects. It will largely depend on its fulness for its success, and the request of the promoters in asking for two copies of a journal in exchange cannot be considered unreasonable.

A French translation of the Right Hon. Professor Huxley's "Science and Religion" has been added to Messrs. Baillière's "Bibliothèque Scientifique Contemporaine."

MR. ELLIOT STOCK has just issued a literal transcription of Captain Cook's Journal during his first voyage round the world, made in H.M. Bark "Endeavour," 1768–71. This is the first occasion on which the complete original MS. has been published, and it is edited, with notes and introduction, by Captain W. J. L. Wharton.

MESSRS. CHAPMAN & HALL have published a new edition of Rev. H. N. Hutchinson's "Extinct Monsters." A few of the restorations by Mr. Smit have been re-drawn and improved, and a large number of woodcuts have been added to the text.

THE Dorset Natural History and Antiquarian Field Club has issued its thirteenth volume (dated 1892). The chief things in this portly "Proceedings" (244 pp.) concerning Natural Science are, a paper by the president, Mr. Mansel-Pleydell, on the occurrence of *Lamprothamnus alopecuroides*, Braun, a Chara for which only one other British locality is known; a paper on "Some Monstrosities of *Littorina rudis*, Maton," by E. R. Sykes, in which all the specimens discussed were gathered in the Fleet Backwater, where the water becomes slightly freshened; and a contribution from the Rev. O. Pickard-Cambridge on the British Species of False-Scorpions. This last is practically a monograph, is illustrated by three excellent plates, and should prove of great assistance to those who devote themselves to Chelifers.

NEWS OF UNIVERSITIES, MUSEUMS, AND SOCIETIES.

Mr. J. Ward, of Derby, has been appointed Curator of the Cardiff Museum.

Dr. Hermann von Ihering has been appointed Curator of the Zoological Department of the San Paulo Museum, Brazil.

Mr. Frederic V. Coville, of the Agricultural Department at Washington, U.S., has been promoted to the directorship of the Botanical Division, which recently became vacant through the death of Dr. Vasey.

Professor H. Newell Martin has resigned the Chair of Biology in the Johns Hopkins University, Baltimore, which he has held for seventeen years. We regret to add that the Professor's ill-health has necessitated this course.

Mr. John H. Cooke, B.Sc., Lecturer on Mathematics in the Maltese University, has been appointed to lecture on Geology in the same institution. Mr. Cooke will also have charge of the fossils and rock-specimens in the Museum of the University, which have hitherto been much neglected. Fortunately, nearly all the known Maltese fossils of importance are preserved in the British Museum.

The University of Edinburgh has conferred the honorary degree of LL.D. upon Dr. Ramsay H. Traquair, F.R.S., the eminent ichthyologist. The University of London has promoted Mr. J. W. Gregory, B.Sc., of the British Museum, to the degree of D.Sc.

There are several newly-appointed Professors of Botany. Mr. W. Bottomley succeeds to the Professorship at King's College, London. Dr. N. Wille, of Aas, has been appointed Professor in the University of Christiania and Director of the Botanic Gardens. Dr. Ferdinand Pax, formerly Privat-docent in the University of Breslau, has succeeded the late Professor Prantl as ordinary Professor and Director of the Botanic Gardens in that University. Professor M. Möbius, of Heidelberg, has been appointed Librarian and Teacher of Botany at the Senckenberg Institute at Frankfort, in succession to the late Dr. Jännicke. Dr. H. Mayr is the new ordinary Professor in the Forestry School at Munich. Mr. Percy Groom has been appointed Demonstrator in the University of Oxford, and Dr. Dreyer, of St. Galle, takes the place of Dr. A. Koch as Assistant in the Plant-Physiological Institute in the University of Göttingen. At Göttingen, also, the position of Assistant in the Botanical Museum and Garden, vacated by Dr. Hallier, is now filled by Dr. Giessler, formerly Assistant in the Botanic Institute at the University of Jena. Dr. Hallier, it may be added, has been appointed Assistant in the Botanic Garden at Buitenzorg, in Java.

AT a meeting held at the House of Commons on May 11, the Committee who have prepared a draft charter for a Welsh University explained their views to the Members for Wales. It is proposed that residence in the new University shall be essential for graduation, but it is hoped that a large number of valuable scholarships will be available for competition.

THE sum of £3,500 has been voted by the University for the improvement of the Oxford Botanic Gardens. The glass houses, according to Professor Vines, are in a very bad state of repair, besides being inadequate and old-fashioned. When the gardens were leased from Magdalen in 1876, it was stipulated the University should execute repairs at the cost of £5,580, but of this sum only £2,000 have hitherto been expended. The present vote is, therefore, a somewhat tardy fulfilment of the obligation.

A NEW American Marine Biological Laboratory, under the direction of Professors C. L. Edwards and A. J. Smith, is being founded at Galveston by the University of Texas. We have also received a circular from Messrs. Sinel and Hornell, of Jersey, announcing the establishment of a Biological Station in connection with their business in the Channel Islands.

THE Yale University, New Haven, U.S.A., is publishing bibliographies of the writings of its Professors. A useful list of the numerous contributions of Professor O. C. Marsh to Vertebrate Palæontology (1861–1892) is to hand.

RESEARCH as a part of Technical Education is promoted, we are glad to see, by some of the County Councils. We have just received from Dr. William Somerville a "Report on Manurial Trials in the County of Northumberland," which gives some useful results from a number of experiments on various artificial manures.

AMONG recent bequests, it is reported that the Marquis Ricci of Genoa has left a large sum for the foundation of a new scientific institution in his native city. The late Earl of Derby bequeathed £2,000 each to the Royal Society and Royal Institution of London.

MR. F. BRADY, C.E., has presented to the British Museum portions of the cores of Carboniferous rocks from the Dover boring. Several pieces of shale contain plant-remains sufficiently well preserved for identification, and they are now exhibited in the gallery of fossil plants at South Kensington.

THE Manchester Museum has just added to its series of useful handbooks a "Catalogue of the Types and Figured Specimens in the Geological Department." The Catalogue is reprinted from the Report of the Museums' Association for 1892. We have also received a second edition of Professor Marshall's "Outline Classification of the Animal Kingdom," which is now supplemented by an "Outline Classification of the Vegetable Kingdom" by Professor Weiss.

WE hear disquieting news from Brighton, where the Corporation is considering proposals for the enlargement of the museum. The Booth collection of birds, which was wisely placed by the founder in a situation as far as possible from the sea air and town smoke, is, we understand, in danger of being removed to the midst of the town, close to the shore. The Natural History collections in the museum, including the unique series of Sussex Cretaceous fossils, are also destined to be very inadequately provided for, judging from the plans we have had the privilege of inspecting. Surely, if the members of the Corporation committee are inexperienced in such matters and cannot obtain the advice of any resident naturalists, it is their duty to secure the services of an expert. We hope that wiser counsels will prevail.

We have received the third part of the second volume of the *Actes de la Société Scientifique du Chili*. Dr. Fernand Lataste contributes most of the small zoological notes, and another instalment of Borne's memoir on *Latrodectus* is published.

An excellent summary of the work of the Scientific Societies of Australia during the past year appears in the recently-issued *Year Book of Australia* for 1893. One new society was founded at Bathurst, New South Wales.

The Cothenius Medal of the German Leopold-Caroline Academy of Naturalists has been awarded to Professor Dr. Adolf Fick of Würzburg, in acknowledgment of his researches in the physiology of muscle.

The Geographical Society of Berlin has bestowed the Humboldt medal upon Dr. John Murray, of Edinburgh; and the Geographical Society of Paris has awarded a gold medal to Dr. Fridjof Nansen.

The two gold medals of the Royal Geographical Society have been awarded this year to Mr. Frederick C. Selous and Mr. Woodville Rockhill. Mr. Selous is the well-known hunter who has contributed so much to our knowledge of the "big game" of Africa. Mr. Rockhill is an American who has made many careful surveys of the "Land of the Lamas." The other awards of the Society are as follows:—The Murchison grant to Mr. R. W. Senior, of the Indian Survey; the Gill memorial to Mr. H. O. Forbes, for researches in New Guinea and the Malay Archipelago; and the Cuthbert Peek grant to Mr. Charles Hose for his explorations in Sarawak, Borneo.

Some months ago we referred to the fact that the Royal Geographical Society had opened its doors to women Fellows. This innovation, which was decided by the Council, instead of by a Special General Meeting of the Fellows, was looked upon as illegal and outside the jurisdiction of the Council under the Charter. A Special General Meeting, therefore, was called on April 24 last to consider the matter. Some 250 Fellows were present, and after an animated discussion, the question of admission of women as Fellows was negatived by 145 to 104. Although we heartily approve of the action of the Fellows, we are sorry to hear of this result, as we feel that the women who have been already elected Fellows of the Society have considerably more claims to the Fellowship than one-half of the male existing members. There has been for some time past much grumbling with regard to the difficulty experienced in getting a seat when some distinguished traveller is discoursing for their information, the reason being that so many ladies are admitted as visitors that numbers of the Fellows themselves are crowded out. Several circumstances account for this, one of which is that most of the meetings of the Geographical Society partake more of the evening "At Home" than of the scientific nature. At the Annual Meeting of the Society on May 29, we understand that Sir Mountstuart Grant-Duff resigns the Presidency, and Mr. Clements Markham is nominated to succeed him.

In the list of fifteen candidates selected this year by the Council of the Royal Society of London for election to the Fellowship, Natural Science is represented by the following names:—Professor J. Cossar Ewart, Dr. W. T. Gairdner, Sir Henry H. Howorth, Mr. E. T. Newton, Mr. C. S. Sherrington, Professor E. C. Stirling, Professor J. W. H. Trail, and Dr. A. R. Wallace. The list will be submitted to a meeting of the Fellows on June 1.

AT the Soirée of the Royal Society of London on May 10, there was little novelty in the Natural History Exhibits. Colonel Swinhoe showed a series of butterflies illustrating protective mimicry; Dr. G. H. Fowler exhibited a series of oyster shells, to illustrate the various modifications and rate of growth; the Zoological Society of London contributed a collection of lepidopterous insects reared in the insect house, and the Marine Biological Station sent some marine invertebrata from Plymouth; Dr. D. Sharp illustrated the sound-producing apparatus of ants; Professor Williamson showed the microscopic structure of some Carboniferous plants, and Mr. E. Wethered the micro-organisms in limestone. Mr. H. O. Forbes exhibited bird-remains from New Zealand and the Chatham Islands, and Mr. E. T. Newton had some casts of the skulls of the extinct Triassic reptiles discovered near Elgin.

AT the Annual Meeting of the Linnean Society on May 24, the officers were re-elected, Professor Charles Stewart retaining the presidency for another year. The newly-issued part of the *Journal* of the Society (Zoology, vol. xxiv., no. 154) contains a paper on the Buprestidæ of Japan, by G. Lewis; descriptions of Crinoids from the Sahul Bank, North Australia, by Professor Jeffrey Bell, and of Land-shells from Borneo, by Edgar A. Smith; a discussion of the affinities of the genus *Madrepora*, by George Brook; and on two new species of *Rhax*, by H. M. Bernard.

THE *Quarterly Journal of the Geological Society* (vol. xlix., part 2), which appeared in the last few days of April, is noteworthy as containing a paper on bivalved mollusca—a subject comparatively neglected in these recent years. It is very encouraging to find, too, that the Society has not been niggardly in regard to illustrations, for no less than four plates, containing 60 figures, are devoted to Dr. Wheelton Hind's paper on *Anthracoptera* and *Anthracomya*. Dr. G. J. Hinde contributes two short papers on Radiolarian rocks; the Rev. P. B. Brodie records some Cestraciont remains from the Keuper of Shrewley; and Mr. Lydekker has a note on a Dinosaurian Vertebra from the Wealden. Mr. Wethered continues his work on the singular structure he refers to *Girvanella*, but we are not prepared yet to consider this under the head of palæontology. Petrology is represented by Mr. Simmons, "On the Petrography of Capraja"; by Miss Raisin, "On the Variolite of the Lleyn"; while Professor Judd returns once more to the controversy, carried on between Sir A. Geikie and himself, over the succession of the rocks in Skye. Messrs. Fox and Teall describe "Some Coast-sections at the Lizard"; and foreign geology is treated of by Lieut. Frederick, on the New Hebrides group, and Mr. Power, on the Pambula gold deposits, New South Wales. In the May number, too, as is the custom, the President's Address is printed *in extenso*, together with the Annual Report, with obituary of deceased Fellows. Mr. Hudleston in his address has given a critical *resumé* of the "Recent work of the Geological Society," and also a readable account of the *Quarterly Journal* for 1892, while at the same time briefly sketching British geology published elsewhere.

THE Trinidad Field Naturalists' Club has issued pt. 7 of its first volume of Proceedings. Mr. Oldfield Thomas contributes a list of the known mammals of Trinidad, with valuable hints for collectors who can still add greatly to knowledge of the subject. Mr. J. H. Collens records some personal observations on the Trinidad manatee, which is now almost extinct; and Mr. W. M. Crowfoot furnishes a preliminary list of Trinidad butterflies. There are other minor communications, and the club appears to be in a very prosperous condition.

OBITUARY.

JAMES WOOD MASON.

WE regret to announce the death of Professor James Wood Mason, which took place at sea in the middle of May at the early age of forty-nine. The deceased naturalist had for some time been in failing health. He went to India in 1869 as Assistant in the Indian Museum, Calcutta, and in 1885 succeeded Dr. J. Anderson as Superintendent of that institution, and also as Professor of Physiology in the Medical College. In addition to his work in Calcutta, he undertook two expeditions (in 1871 and 1873) to the Andaman and Nicobar Islands, where he made considerable collections, and in 1880 he investigated, on behalf of the Government, the life-history of the destructive "tea-bug" in Assam. He made a special study of crustacea and insects, paying particular attention to the Phasmidæ and Mantidæ, a work on the latter group being unfortunately cut short by his untimely death. His contributions to science are contained in numerous papers in the *Journ. Asiat. Soc. Bengal*, the *Ann. Mag. Nat. Hist.*, and other journals.

DR. MOLESCHOTT, Professor of Physiology in the University of Rome, died on May 20 at the age of seventy. A Dutchman by birth, he became Professor of Physiology, Anatomy, and Anthropology at Heidelberg in 1847, removing nine years later to Zurich, thence to Turin, and in 1879 to Rome.

MR. WILLIAM COTTON OSWELL, the well-known African traveller, died on May 1 at the age of seventy-five. The death is also announced of the traveller and botanist, Dr. J. Braun, who had been exploring Madagascar.

AMONG other recent deaths we also have to record those of Mr. Edward Vivian, of Torquay, Editor of MacEnery's "Cavern Researches" (1859); of Dr. John Passerini, Director of the Botanic Gardens in the University of Parma; of Professor Ferdinand Senft, the veteran geologist; of Professor Robert Hartmann, the German anatomist and anthropologist; and of the entomologists, E. G. Honrath and J. Bigot.

CORRESPONDENCE.

Natural Selection and Lamarckism: A Correction.

On page 337, line 17, I would ask the reader to substitute the word "evident" for the word "admitted," as I find I was in error in supposing that "it is admitted" that Natural Selection would evolve a general or widely-diffused sense of touch. Mr. Spencer's views on the matter are more extreme and exclusive than I gathered from his article. In the latest number of the *Contemporary Review*, he denies that either general or special sensitiveness of the skin results from Natural Selection. I am sorry that I misinterpreted his view on this point; but I fail to understand how a great philosopher can accept Natural Selection as a factor of evolution and yet suppose that it would play no important part in developing a sense so necessary for safety and survival as that of touch.

Wm. Platt Ball.

Mr. Hick and Calamostachys.

In a paper which appears in the number of Natural Science for May (vol. ii., pp. 354–359), Mr. Hick has, if I mistake not, brought before the public for the third time his views respecting the structure and affinities of *Calamostachys Binneyana*. I am not disposed to enter upon what, to be of any value, must be a prolonged and detailed controversy; but were I to allow this paper to be circulated unnoticed by me, such a course might be equivalent to an admission that I had made serious blunders, which Mr. Hick had not only corrected, but that his evidence furnishes what he claims to be "a complete solution of the difficulty thus presented." This claim I must definitely decline to recognise. I fear that the difficulties in the way of obtaining this solution are greater than Mr. Hick realises.

For several months my friend, Dr. Scott, and I have been working together, at the Joddrell Laboratory at the Kew Gardens, re-investigating the entire subject of the Carboniferous Calamariæ, including under that comprehensive term the two genera *Calamites* and *Calamostachys*, having as material for our investigation such a collection of sections of these objects as has no existence outside my cabinets. We hope to place before the Royal Society the results of these minute and careful studies, elaborately illustrated, before the close of the present year. This memoir will be our true answer to Mr. Hick's statements. Meanwhile, that gentleman must excuse me if I decline to recognise the accuracy of a considerable number of statements contained in his paper, or admit that he has settled the controverted questions so conclusively as he claims to have done.

W. C. Williamson.

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All communications for the Editor *to be addressed to the* Editorial Offices, *now removed to* 5 John Street, Bedford Row, London, W.C.

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